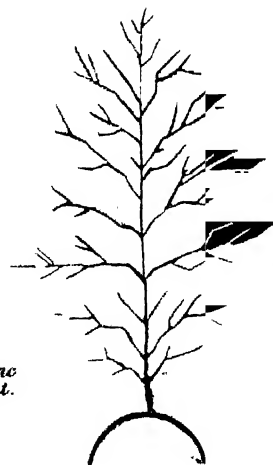
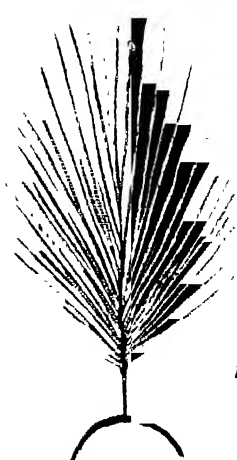
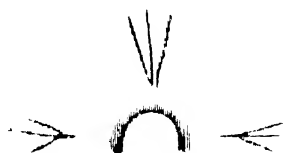


*Singular Figure
of Saturn*



*Electric
Light.*



A
JOURNAL
OF
NATURAL PHILOSOPHY,
CHEMISTRY,
AND
THE ARTS.

VOL. XIII.

Illustrated with Engravings.

B.^Y WILLIAM NICHOLSON.

LONDON:
PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR, FOR
THE AUTHOR,
AND SOLD BY HIM AT NO. 10, SOHO-SQUARE;
AND BY
H. D. SYMONDS, PATERNOSTER-ROW.

1806.

PREFACE.

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Of Foreign Works, M M. Callias and Co.; Colonel Skioeldebrand; M. Debue; M. Favre; Prof. Playfair; M. W. A. Cadell; M. Rosseau and Genon; M. Riffant; Professor Heeven; Lagrange; Curaudeau; Sorbie; Humboldt; Gay Lussac; Drappier; P. S. Girarey; Messrs. Reynard and Facquer; Bucholz; Hermestadt; Biemontier; M. P. Dispan; M. Poideyin; Haufman.

And of English Memoirs abridged or extracted, William Herschel, L. L. D. F. R. S.; J. Horsburgh, Esq.; Henry Cavendish, F. R. S.; C. Hatchett, Esq.; Dr. Balfour; M. Flinders, Esq.; W. H. Wollaston, M. D. Sec. R. S.; Rev. W. Gregor; Mr. W. Shirreff; Rev. Dr. W. Richardson; Benjamin Smith Barton, M. D.; Dr. Holme; Sir James Hall, Bart. F. R. S.; Mr. B. Gibson; T. A. Knight, Esq. F. R. S.; Rt. Rev. Bishop Madison; Mr. B. Gibson; Mr. Thomas Eamshaw.

Soho Square, London, May 1, 1806.

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TO THE BINDER.

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY.
AND
THE ARTS.

JANUARY, 1806.

ARTICLE I.

On the Cause of Fairy Rings. By JOHN GOUGH, Esq.

To Mr. NICHOLSON,

SIR,

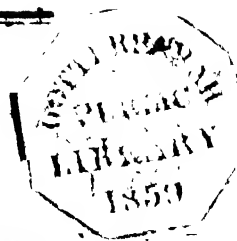
Middleshaw, December 7, 1805.

YOU published in the first page of your ninth volume in octavo, a letter to me from the Rev. Jonathan Wilson, vicar of Biddulph, in which the appearance of a patch of ground recently blasted and torn up by lightning was described. The observations of this ingenious and accurate gentleman promised to throw light on the natural history of fairy-rings, provided they were continued, and in this expectation, I took the liberty in a note subjoined to the copy of his letter printed by you, to request his future remarks on the subject, drawn from repeated inspection of the place affected by the lightning. The following is an extract of a letter from Mr. Wilson, containing his observations relative to the subsequent appearances of the patch, with some thoughts which are certainly an improvement in one theory of fairy-rings, that has received the patronage of some writers. This letter is dated November 1, 1805, and after some prefatory matter, proceeds thus,

VOL. XIII.—JANUARY, 1806.

R

" In



FAIRY RINGS.

Mr. Willson's remarks begin. "In consequence of your esteemed favour of the 14th of August, 1804, I went on the 2nd of September following, to view the place which the lightning had struck, being accompanied by the farmer of the grounds. The affected spot was not then very easy to be distinguished, as the injured thistles were generally overtopped at the time, but we had no doubt of its true situation, upon finding the place where we formerly dug in pursuit of the imaginary stone. Some dead grass appeared, but it was confined to the space within which the roots had been plowed up by the electric fluid. The verdure was undoubtedly brighter about the hole, and the farmer was willing to attribute the flourishing state of this circle of herbage to the lightning; but for my part, I ascribed it to what had dropped from his cows, rather than any thing that had fallen from the clouds.

Slight vestiges of the lightning. "I have not been able to perceive the least difference between the part struck, and the rest of the field, during the course of the present year; my observations must therefore be acknowledged not to favour the hypothesis, which supposes fairy-rings to be formed at first by lightning.

The explanation by grubs improved. "I never saw a fairy-ring, and therefore may seem badly qualified to write on the subject; but from what I have read, it appears to me, that the circle of decayed grass is caused by the innumerable grubs, which are said to lay concealed under the ring among the roots of the herbage; I also suppose, that the funguses commonly seen on fairy-rings, give a preference to these circles on account of the abundance of dead vegetable matter to be found in them; amongst which various species of fungi are known to grow. To this I may add, that the interior circle of dark green grass is owing to the dung, and ploughing of those animals in the preceding year; and the reason which compels these grubs or their offspring to push forward from the centre, seems to be this; every creature we know of has an aversion to working in its own excrement, or that of its own species."

Reflections on these observations. The observations of Mr. Willson, stated above, seem to demonstrate, that a patch of herbage is not invariably converted into a fairy-ring by a powerful stroke of lightning; consequently if electric discharges from the atmosphere be the primary causes of these circles, they require the assistance of some peculiarity in the soil to give permanency to the appearance,

ance. Moreover the circular figure of these phenomena ^{Difficulties of the electrical theory.} embarrasses the electrical hypothesis with a second difficulty; for the tracts of discoloured grass, actually produced by lightning, are but seldom bent into rings, as they more frequently assume a zigzag, or else a ramified form, and are, I believe, of but short duration; which shews, the roots of the herbage are not destroyed, unless where the earth is torn up. ^{Fungi not the cause of fairy rings.} The theory which attributes these circles of withered grass to the running of a fungus, has little or no foundation; because these imperfect plants, generally speaking, attach themselves to dead vegetables, consequently their presence in fairy-rings is nothing more than an appearance which is subsequent to the destruction of the herbage upon them. As for the lively ^{Superior verdure of these rings explained.} verdure of the grass on the interior edges of these circles, I believe it may be explained upon general principles, without the agency of lightning or fungi. For if the herbage of a patch of ground be destroyed root and branch by a cause which does not remove the remains of it, the place will be covered, in process of time, with a fresh crop of plants, possessing superior luxuriance and verdure. ^{Dead plants, &c. manure the ground.} The causes of this vigorous vegetation appear to be the following; the dead roots and stems rot and manure the soil which produced them: this source of fertility receives an additional supply from a succession of fungi, which grow and decay on the surface of the ground, as well as from the excrement and exuvia of the grubs, which take up their abode among the withered roots; lastly, the soil is rendered more porous by ^{The air increases vegetation by acting on the roots.} the decay of the vegetable remains, and thereby becomes more permeable to the air, which increases its fertility not a little. The last position seems to be confirmed by the circumstance of plants thriving better in unglazed, than in glazed flower pots. ^{Proofs of the preceding theory.} The following facts may be adduced in corroboration of what has been here advanced. A small piece of ground was covered, in June, with common salt, which had been rubbed upon the corps of a drowned man; the herbage of this place died in a short time; but was succeeded the next summer by a new crop, the verdure of which distinguished it for some years from the surrounding grass. As common salt is esteemed a manure, perhaps the following instance will be called preferable to the former. Many woods in this country, especially those about Windermere, are cut

down about once in fourteen years, and converted into charcoal, for the use of the iron works. This is done by throwing the branches into large heaps, which are then covered with green turf, and set on fire. These piles of wood continue burning for several days, in consequence of which, the roots in the ground beneath them are completely charred; and the pit-head, as the place is called, has no verdure left upon it. The loss however is repaired in the course of a few years by nature herself, where art does not interfere, and the spot is clothed with a fresh coat of herbage, consisting of herbs remarkable for their size and flourishing appearance. This instance of vigorous vegetation on ground that has been completely burned, in all probability, is occasioned by the texture of the soil; which is adapted to retain moisture, and admit the air; unless we suppose the incorruptible substance of charcoal to afford a species of manure.

The preceding hints may perhaps incite some of your readers to study the natural history of fairy-rings with greater diligence, especially to search after the true cause which blasts these circles; for when this is discovered, we shall be able to re-clothe them with superior verdure, on rational principles.

I remain, &c.

JOHN GOUGH:

II.

Observations on the singular Figure of the Planet Saturn. By
WILLIAM HERSCHEL, LL. D. F. R. S.*

THERE is not perhaps another object in the heavens that presents us with such a variety of extraordinary phenomena as the planet Saturn: a magnificent globe, encompassed by: stupendous double ring: attended by seven satellites: ornamented with equatorial belts: compressed at the poles: turning upon its axis: mutually eclipsing its ring and satellites, and eclipsed by them; the most distant of the rings also turning upon its axis, and the same taking place with the farthest of the satellites: all the parts of the system of Saturn occasionally

Examination of
the striking phe-
nomena of the
planet Saturn.

* Philosophical Transactions, 1805.

reflecting light to each other: the rings and moons illuminating the nights of the Saturnian: the globe and satellites enlightening the dark parts of the rings: and the planet and rings throwing back the sun's beams upon the moons, when they are deprived of them at the time of their conjunctions.

• It must be confessed that a detail of circumstances like these, appears to leave hardly any room for addition, and yet the following observations will prove that there is a singularity left, which distinguishes the figure of Saturn from that of all the other planets.

It has already been mentioned on a former occasion, that so far back as the year 1776 I perceived that the body of Saturn was not exactly round; and when I found in the year 1781 that it was flattened at the poles at least as much as Jupiter, I was insensibly diverted from a more critical attention to the rest of the figure. Prepossessed with its being spheroidal, I measured the equatorial and polar diameters in the year 1789, and supposed there could be no other particularity to remark in the figure of the planet. When I perceived a certain irregularity in other parts of the body, it was generally ascribed to the interference of the ring, which prevents a complete view of its whole contour; and in this error I might still have remained, had not a late examination of the powers of my ten-foot telescope convinced me that I ought to rely with the greatest confidence upon the truth of its representations of the most minute objects I inspected.

Reasons why the greater peculiarities of its figure were overlooked:

The following observations, in which the singular figure of Saturn is fully investigated, contain many remarks on the rest of the appearances that may be seen when this beautiful planet is examined with attention; and though they are not immediately necessary to my present subject, I thought it right to retain them, as they show the degree of distinctness and precision of the action of the telescope, and the clearness of the atmosphere at the time of observation.

April 12, 1805. With a new 7-foot mirror of extraordinary distinctness, I examined the planet Saturn. The ring reflects more light than the body, and with a power of 570 the colour of the body becomes yellowish, while that of the ring remains more white. This gives us an opportunity to distinguish the ring.

Very perfect observation, in which the circumference was seen to be flattened in four reg ons.

ring from the body, in that part where it crosses the disk, by means of the difference in the colour of the reflected light. I saw the quintuple belt, and the flattening of the body at the polar regions; I could also perceive the vacant space between the two rings.

Observations on Saturn by which its singular figure is ascertained.

The flattening of the polar regions is not in that gradual manner as with Jupiter, it seems not to begin till at a high latitude, and there to be more sudden than it is towards the poles of Jupiter. I have often made the same observation before, but do not remember to have recorded it any where.

April 18; ten-feet reflector, power 300. The air is very favourable, and I see the planet extremely well defined. The shadow of the ring is very black in its extent over the disk south of the ring, where I see it all the way with great distinctness.

The usual belts are on the body of Saturn; they cover a much larger zone than the belts on Jupiter generally take up, as may be seen in the figure I have given in Plate I.; and also in a former representation of the same belts in 1794.*

The figure of the body of Saturn, as I see it at present, is certainly different from the spheroidal figure of Jupiter. The curvature is greatest in a high latitude.

I took a measure of the situation of the four points of the greatest curvature, with my angular micrometer, and power 527. When the cross of the micrometer passed through all the four points, the angle which gives the double latitude of two of the points, one being north the other south of the ring, or equator, was $93^{\circ} 16'$. The latitude therefore of the four points is $46^{\circ} 38'$; it is there the greatest curvature takes place. As neither of the cross wires can be in the parallel, it makes the measure so difficult to take, that very great accuracy cannot be expected.

The most northern belt comes up to the place where the ring of Saturn passes behind the body, but the belt is bent in a contrary direction being concave to the north, on account of its crossing the body on the side turned towards us, and the north pole being in view.

There is a very dark, but narrow shadow of the body upon the following part of the ring, which as it were cuts off the ring from the body.

* See Phil. Transf. for 1794, Table VI. page 32.

The shadow of the ring on the body, which I see south of the ring, grows a little broader on both sides near the margin of the disk. . . .

The division between the two rings is dark, like the vacant space, between the ansæ, but not black like the shadow I have described.

There are four satellites on the preceding side near the ring; the largest and another are north-preceding; the other two are nearly preceding.

April 19. I viewed the planet Saturn with a new 7-feet telescope, both mirrors of which are very perfect. I saw all the phenomena as described last night, except the satellites, which had changed their situation; four of them being on the following side. This telescope however is not equal to the 10-feet one.

The remarkable figure of Saturn admits of no doubt: when our particular attention is once drawn to an object, we see things at first sight that would otherwise have escaped our notice.

10-feet reflector, power 400. The night is beautifully clear, and the planet near the meridian. The figure of Saturn is somewhat like a square or rather parallelogram, with the four corners rounded off deeply, but not so much as to bring it to a spheroid. I see it in perfection.

The four satellites that were last night on the preceding, are now on the following side, and are very bright.

I took a measure of the position of the four points of the greatest curvature, and found it $91^{\circ} 29'$. This gives their latitude $45^{\circ} 44', 5$. I believe this measure to be pretty accurate. I set first the fixed thread to one of the lines, by keeping the north-preceding and south-following two points in the thread; then adjusted the other thread in the same manner to the south-preceding and north-following points.

May 5, 1805. I directed my 20-feet telescope to Saturn, and, with a power of about 300, saw the planet perfectly well defined, the evening being remarkably clear. The shadow of the ring on the body is quite black. All the other phenomena are very distinct.

The figure of the planet is certainly not spheroidal, like that of Mars and Jupiter. The curvature is less on the equator

Observations on and on the poles than at the latitude of about 45 degrees. Saturn, by which its singular figure is ascertained. The equatorial diameter is however considerably greater than the polar.

In order to have the testimony of all my instruments, on the subject of the structure of the planet Saturn, I had prepared the 40-foot reflector for observing it in the meridian. I used a magnifying power of 360, and saw its form exactly as I had seen it in the 10 and 20-foot instruments. The planet is flattened at the poles, but the spheroid that would arise from this flattening is modified by some other cause, which I suppose to be the attraction of the ring. It resembles a parallelogram, one side whereof is the equatorial, the other the polar diameter, with the four corners rounded off so as to leave both the equatorial and polar regions flatter than they would be in the regular spheroidal figure.

The planet Jupiter being by this time got up to a considerable altitude, I viewed it alternately with Saturn in the 10-foot reflector, with a power of 500. The outlines of the figure of Saturn are as described in the observation of the 40-foot telescope; but those of Jupiter are such as to give a greater curvature both to the polar and equatorial regions than takes place at the poles or equator of Saturn which are comparatively much flatter.

May 12. I viewed Saturn and Jupiter alternately with my large 10-foot telescope of 24 inches aperture; and saw plainly that the former planet differs much in figure from the latter.

The temperature of the air is so changeable that no large mirror can act well.

May 13. 10-foot reflector, power 300. The shadow of the ring upon the body, and of the body upon the ring, are very black, and not of the dusky colour of the heavens about the planet, or of the space between the ring and planet, and between the two rings. The north-following part of the ring, close to the planet, is as it were cut off by the shadow of the body; and the shadow of the ring lies south of it, but close to the projection of the ring.

The planet is of the form described in the observation of the 40-foot telescope; I see it so distinctly that there can be no doubt of it. By the appearance, I should think the
points

points of the greatest curvature not to be so far north as 45 degrees. Observations on Saturn, by which its singular figure is ascertained.

The evening being very calm and clear, I took a measure of their situation, which gives the latitude of the greatest curvature, $45^{\circ} 21'$. A second measure gives $45^{\circ} 41'$.

Jupiter being now at a considerable altitude, I have viewed it alternately with Saturn. The figure of the two planets is decidedly different. The flattening at the poles and on the equator of Saturn is much greater than it is on Jupiter, but the curvature at the latitude of from 40 to 48° on Jupiter is less than on Saturn.

I repeated these alternate observations many times, and the oftener I compared the two planets together, the more striking was their different structure.

May 26. 10-feet reflector. With a parallel thread micrometer and a magnifying power of 400, I took two measures of the diameter of the points of greatest curvature. A mean of them gave 64,3 divisions = $11'' 98$. After this, I took also two measures of the equatorial diameter, and a mean of them gave 60,5 divisions = $11'' 27$; but the equatorial measures are probably too small.

To judge by a view of the planet, I should suppose the latitude of the greatest curvature to be less than 45 degrees. The eye will also distinguish the difference in the three diameters of Saturn. That which passes through the points of the greatest curvature is the largest; the equatorial the next, and the polar diameter is the smallest.

May 27. The evening being very favourable, I took again two measures of the diameter between the points of greatest curvature, a mean of which was 63,8 divisions = $11'' 68$. Two measures of the equatorial diameter gave 61,3 divisions = $11'' 44$.

June 1. It occurred to me that a more accurate measure might be had of the latitude in which the greatest curvature takes place, by setting the fixed thread of the micrometer to the direction of the ring of Saturn, which may be done with great accuracy. The two following measures were taken in this manner, and are more satisfactory than I had taken before. The first gave the latitude of the south-preceding point of greatest curvature $43^{\circ} 26'$; and the second $43^{\circ} 13'$. A mean of the two will be $43^{\circ} 20'$.

June

Observations on June 2. I viewed Jupiter and Saturn alternately with a Saturn, by magnifying power of only 300, that the convexity of the which its singular figure is ascertained. eye-glass might occasion no deception, and found the form of the two planets to differ in the manner that has been described.

With 200 I saw the difference very plainly; and even with 160 it was sufficiently visible to admit of no doubt. These low powers show the figure of the planets perfectly well, for as the field of view is enlarged, and the motion of the objects in passing is lessened, we are more at liberty to fix our attention upon them.

I compared the telescopic appearance of Saturn with a figure drawn by the measures I have taken, combined with the proportion between the equatorial and polar diameters determined in the year 1789; * and found that, in order to be a perfect resemblance, my figure required some small reduction of the longest diameter, so as to bring it nearly to agree with the measures taken the 27th of May. When I had made the necessary alteration, my artificial Saturn was again compared with the telescopic representation of the planet, and I was then satisfied that it had all the correctness of which a judgment of the eye is capable. An exact copy of it is given in Plate IX. The dimensions of it in proportional parts are,

• The diameter of the greatest curvature	-	36
The equatorial diameter	- - - -	35
The polar diameter	- - - -	32
Latitude of the longest diameter	-	43° 20.'

The foregoing observations of the figure of the body of Saturn will lead to some intricate researches, by which the quantity of matter in the ring, and its solidity, may be in some measure ascertained. They also afford a new instance of the effect of gravitation on the figure of planets; for in the case of Saturn, we shall have to consider the opposite influence of two centripetal and two centrifugal forces: the rotation of both the ring and planet having been ascertained in some of my former Papers.

* See Phil. Trans. for 1790, page. 17.

III.

Facts and Observations on the medical Respiration of gaseous Oxide of Azote. In a Letter from Dr. BEDDOES.

To Mr. NICHOLSON.

SIR,

DR. Pfaff's paper on respiration* will probably draw the attention of the scientific towards the gaseous oxide of azote, which has been too much neglected in a medical point of view. I was only sorry to see that he proposes to use it in melancholia. No combination of ideas can be more obvious than the application of an agent which has so frequently proved exhilarating, and never yet been observed to be followed by exhaustion where it did exhilarate, to a complaint, in which depression of spirits is a striking circumstance. But I am apprehensive that the first thoughts of inexperience here (as so often happen) will prove illusory, and that this project will not be followed by the expected advantage in many cases of melancholia. For if it be true that there is no real distinction between mania and melancholia, as far as the sensorium is concerned, and that the vivacity of ideas in melancholia answers to the violence of muscular actions in mania, as I have endeavoured to shew in my *Essays on Health*; is there not ground to apprehend that the actions of the brain, already too strong, will be increased by this gas, or the diseased contemplations rendered more intense?

Dr. Pfaff's experiments on respiration.

Proposes gaseous oxide of azote in melancholy madness.

If there be any state of melancholia in which it may be of service, this will probably happen when the nervous system is falling into debility, in consequence of having been kept too much on the stretch.

Cautions against its unguarded use, from the-
ory;

But I do not here warn against gaseous oxide from mere theory. The manager of a lunatic asylum near Bristol, respectably known to the public, concurred with me some years ago in the opinion which I expressed to him concerning its probable advantage in melancholia; and a patient that had been under his care inhaled it fairly without benefit. The administration was tried in two other cases as fruitlessly: Indeed

ence.

I discontinued it in one, from some indications of an aggravation of the symptoms. I was by this time alive to suspicion, having thought much on the subject, and reasoned myself into the idea that it would often do injury upon the above-mentioned principle. It has long been my opinion, and there are striking observations on record to prove that hydrogen, hydro-carbonate, azotic, or carbonic acid gases, would be more likely to answer in active insanity under whatever form. These observations I shall take occasion to quote hereafter.

Use in palsy of
one kind.

The very first time I witnessed the effects of gaseous oxide on a person in health, I concluded that it would be a remedy in certain cases of palsy. A patient who had emerged from apoplexy with the loss of the power of one side of his body, was accordingly put under a course of the gas. The result completely answered expectation. The case was most carefully watched; and on withholding the gas, the symptoms repeatedly grew worse, and *vice versa*. After the patient's recovery, he was kept under inspection for a considerable time, and did not relapse. This has been confirmed by other results; and in palsy, where the brain is primarily affected, I expect that Dr. Plaff will find either a cure or great relief to follow the use of this gas in a respectable proportion of cases.

In another kind
of palsy.

I have very fairly tried it in palsy apparently from cold, beginning at the extremities and creeping from muscle to muscle, without good or bad effect. There is a case of this kind, related by Dr. Kentish, with the patient's name, and corroborated by testimony superior to all exception in *Considerations on facitious Airs (Johnson)* in which a perfect cure was obtained from oxygen gas; and I have since learned, by experiments carefully repeated before various philosophical observers, that in essential respects, oxygen gas and gaseous oxide act in a very different, nay opposite manner upon the living fibre.

Or oxygen,

These experiments I hope to publish before midsummer.

In dropsies;

From palsy, analogy led me to other cases of debility. I fully tried gaseous oxide in dropsy of the chest (anasarca of the lungs), but without good or bad effect. I was much disappointed, conceiving that in dropsy (at least in one species) we have a paralytic state of the lymphatics. But I have been since assured by a physician, that for some dropsies he has found

found a remedy in this gas. There are dropsies which doubtless depend on excess of exhalant action. These are easily distinguished; and they require bleeding as much as pleurisy.

In debility, arising from residence in hot climates and from ^{in other states of debility.} intense application to business, I have known gaseous oxide completely successful after an infinity of remedies, Bath and other waters, had been tried in vain.

The particulars of these cases are also destined for publication: But I resolved to wait for some years after the use of the gas; for I have found that a single circumstance vitiates a large proportion of our medical records. Patients after an apparent recovery fall again into the same complaint; and there are other considerations, which I shall for the present pass over.

If Mr. Pfaff uses gaseous oxide in palsy, he will probably sooner or later see a phenomenon as extraordinary as any in galvanism, and which after it has been described by a philosopher of high reputation, will become equally celebrated. ^{Gaseous oxide has given voluntary power over palsied parts, while inhaled.} This is the instantaneous restoration of voluntary power over a limb deprived both of motion and feeling by palsy succeeding to apoplexy, while the patient is respiring gaseous oxide. This was witnessed in common with myself, by several respectable persons; and among others by some of your philosophical acquaintance, if I do not mistake. ^{It was in the case of Mr. G. a member of the last parliament, who completely recovered: But as other means were afterwards adopted, I do not impute the result to the gas, which however, when used alone, was visibly of great service: for I have no idea of claiming for a remedy under scrutiny any cure, if other powers have been called in at the same time.}

I transmit these observations to you, Sir, in preference to the Editor of any Medical Journal, because I think them likely ^{First wished for effect of this statement.} to meet the eye of Dr. Pfaff sooner in your Journal. I should be extremely sorry that he should set out wrong in his trials, because the fault will be imputed to the power itself, and not to its misapplication; and the disabled will still be left to languish and be cut off, notwithstanding we have a remedy at hand.

I have another reason. I most sincerely wish any thing I ^{A second.} could say would hasten the period, which *must* arrive, when medical science shall not be merely what the Germans call a

Brod.

Brod-wissenschaft, or pursued only for a livelihood. If philosophical men without a profession would take it up, it is I think certain, that it must soon become both more efficient and more liberal. Any study is capable of interesting the feelings; and most surely that of the laws of the organic world is as much so as any other. Opportunities of anatomical, chemical, and clinical information are at hand. A person so prepared will, heaven knows, with ardour and industry soon acquire all that is useful in medical practice. Let him then, animated with no other motive than the pure desire of benefiting his fellow men, apply himself to the improvement of medicine. It is impossible that he should not succeed as fully as our Tennants, our Hatchetts, and Chenevix's have done in chemistry; for it is not its inherent difficulty, but collateral circumstances, that retard the progress of this art. Many apothecaries, for example, and old women in general, who are the great controulers of the destiny of physicians, would by no means allow the use of gaseous oxide in palsy, though the patient in the course both of nature and of ordinary medication be sure to die, and perhaps in a very miserable manner. But the philosophical cultivator of medicine, without troubling himself about the good opinion of the one or the other, would proceed on his career under the guidance of the collective light of science and of humanity.

Men of liberal
curiosity ex-
horted to study
medicine.

Anecdote.

N. N. advanced in years, of a thickset stature, and with a short neck, shewed signs of palsy many years ago. The writer of these lines warned his friends of the danger. Concurring in this apprehension, Dr. Ingenhousz proposed to him to inhale oxygen gas, a practice familiar to that accurate philosopher, and by which he hoped the constitution might be recruited. The execution of the idea was deferred. Meanwhile the gaseous oxide was discovered to be respirable, and its power in palsy was to a degree ascertained. The writer now pressed the use of this gas with the utmost earnestness. The patient saw it taken by others: He himself consented to inhale it, when behold! the distress of a lady present, as excited by some apprehended imaginary bad consequences, put off the inhalation. The predicted paralytic seizure arrived: but there was ample time still for the use of the oxide. I proposed that another patient, situated as similarly as possible, should be sought; and that if he consented upon the credit

RESPIRATION OF GASEOUS OXIDE OF AZOTE.

credit of the successful exhibition, and upon my responsibility, to use the gas, the result should determine as to its employment in the case first in question. At the same time, I stated from the average course of paralytic attacks in general not immediately fatal, that a little apparent amendment would take place, and the stroke return with additional violence. My proposal was acknowledged to be highly reasonable; but that plan of routine treatment was followed which is so much more advantageous to the idle and unscientific of our profession than it is to the sick, and the patient died of a return of his complaint. Such is probably the condition of thousands of the diseased at this moment! Rather than use a recently proposed plan not in the Pharmacopœia, or seek a new one in analogy, we persevere in painful or disgusting means, from which, on the faith of long experience, no good of any sort can be expected for the sufferer. May the rising generation of natural philosophers exercise their talents and their benevolence in putting an end to so crying an evil!

I am, Dear Sir,

Respectfully yours,

THOMAS BEDDOES.

Clifton, Dec. 13, 1805.

P. S. A case in your Journal, where a gentleman accustomed to breathe gaseous oxide for amusement, experienced very disagreeable feelings on one particular occasion, seems to me clearly referable to hysteria. Now the trials at the Pneumatic Institution, as related in Mr. Davy's *Researches*, had clearly shewn that in the predisposed, gaseous oxide is a specific for exciting an hysterical paroxysm. Perhaps in the individual whose case is related by himself in the Journal, no obvious predisposition, either temporary or permanent, existed: Nothing to this purpose is stated. But that the affection was simply hysterical cannot I think be doubted by any one conversant both with hysteria and the administration of gaseous oxide. It seems to be strongly marked by that idea of immediate danger, which is so common in hysteria. Dr. Garnet very unnecessarily, and, I believe, very mistakenly, called up the whole Brunoian theory on the emergency. It led him, however, to give cordials; and they were proper. A tea-spoonful of sal volatile, from

Caution regard-
ing particular
subjects.
Quære.

from time to time, would probably have answered without the Brunonian theory. But it is certainly the business of the physician to avoid gaseous oxide in the hysterical, as it is wine in those who labour under acute inflammation. If your correspondent who related his own feelings could specify any cause which might have rendered him nervous, or state the fact whether he was so or not, it would give satisfaction to the present writer, and perhaps also to future inquirers.

Remark on dan-
gerous disorders.

To interdict a remedy because its use requires discrimination, would, in many disorders, be leaving the sick to certain destruction. I imagine that the outcry against such means as gaseous oxide, will arise from those who daily use the most hazardous remedies, and who are enabled to do it without reproach, because they are put into a phial, and the patient and his friends never trouble themselves about the nature of the articles which they are receiving into the stomach.

IV.

Abstract of Observations on a diurnal Variation of the Barometer between the Tropics. By J. HORSBURGH, Esq. In a Letter to HENRY CAVENDISH, Esq. F. R. S. Read March 14, 1803.*

S I R,

Bombay, April, 20, 1804

Tropical varia-
tion of the ba-
rometer.

WHEN I was in London at the conclusion of the year 1801, I had the pleasure of being introduced to you by my friend Mr. Dalrymple, at which time he presented you with some sheets of meteorological observations, with barometer and thermometer, made by me in India, and during a passage from India to England.

Being of opinion that few registers of the barometer are kept at sea, especially in low latitudes, I have been induced to continue my observations since I left England, judging that, even if they were found to be of no utility, they might at least be entertaining to you or other gentlemen, who have been making observations of a similar nature.

During my last voyage I have employed two marine barometers, one made by Troughton, the other by Ramsden,

* Philosophical Transactions, 1805.

and

and a thermometer by Frazer. These were placed exposed to a free current of air in a cabin, where the basons of the barometers were 15 feet above the level of the sea. Tropical variation of the barometer.

The hours at which the heights of the barometers, and thermometers were taken, viz. noon, 4 hours, 10 hours, 12 hours, 14 hours, and 19 hours, were chosen, because at these times the mercury in the barometer had been perceived to be regularly stationary between the tropics, by former observations made in India in 1800 and 1801. It was found that in settled weather in the Indian seas, from 8 A M to noon, the mercury in the barometer was generally stationary, and at the point of greatest elevation; after noon it began to fall, and continued falling till 4 afternoon, at which time it arrived at the lowest point of depression. From 4 or 5 P M the mercury rose again, and continued rising till about 9 or 10 P M, at which time it had again acquired its greatest point of elevation, and continued stationary nearly till midnight; after which it began to fall, till at 1 A M it was again as low as it had been at 4 afternoon preceding; but from this time it rose till 7 or 8 o'clock, when it reached the highest point of elevation, and continued stationary till noon.

Thus was the mercury observed to be subject to a regular elevation and depression twice in every 24 hours in settled weather; and the lowest station was observed to be at about 4 o'clock in the morning and evening. I remarked that the mercury never remained long fixed at this low station, but had a regular tendency to rise from thence till towards 8 in the morning and about 9 in the evening, and from those times continued stationary till noon and midnight.

In unsettled blowing weather, especially at Bombay during the rains, these regular ebbs and flowings of the mercury could not be perceived; but a tendency to them was at some times observable when the weather was more settled.

In the sheets, which I formerly presented to you, were evinced these elevations and depressions twice every 24 hours within the tropics, in steady weather, as had been observed by Mess. Cassan and Peyrouse, by Dr. Balfour of Calcutta, and others. But since my last arrival in India, I have observed that the atmosphere appears to produce a different effect on the barometer at sea from what it does on shore.

Tropical variation of the barometer.

As I am ignorant whether this phenomenon has been noticed by any person before. I will here give you an abstract of my journal, shewing how the barometer has been influenced during the whole time since I left England, which will enable you to form an idea whether I am right in concluding that the barometer is really differently affected at sea from what it is on shore, at those places in India where the observations have been made.

The first sheet begins with the observations made on board ship, in my voyage from London towards Bombay, in the months of April and May, 1802.

From the time of leaving the Land's End, April 19th, the motion of the mercury in barometers was fluctuating and irregular until we were in latitude 26° N, longitude 20° W, on April 29th; the mercury in barometers then became uniform in performing two elevations and two depressions every 24 hours, (which for brevity in mentioning hereafter I will call equatropical motions.) From latitude 26° N to latitude 10° N, the difference of the high and low stations of the mercury in the barometers was not so great, as it was from latitude 10° N across the equator, and from thence to latitude 25° S. Within these last-mentioned limits, the difference of high and low stations of the mercury in the barometers was very considerable, generally from five to nine hundred parts of an inch, both in the daily and nightly motions.

When we reached the latitude of 28° S, longitude 27° W, June 7th, the mercury in barometers no longer adhered to the equatropical motions; but then, as in high north latitudes, its rising and falling became irregular and fluctuating during our run from latitude 28° S, longitude 27° W, (mostly between the parallels of 35° and 36° S,) until we were in latitude 27° S, and longitude 51° E, on the 11th of July. The mercury then began to perform the equatropical motions, and continued them uniformly, during our run from the last-mentioned position, up the Madagascar Archipelago, across the Equator, until our arrival at Bombay July 31st. 1802.

August 6th, 1802. When the barometers were placed on shore in Bombay, the mercury, for the first six days, appeared to have a small tendency towards performing the equatropical motions, but not equally perceptible as when at sea, the difference between the high and low stations of the mercury in
the

the barometers being great to the day we entered the harbour of Bombay. From the 12th of August to the 22d the mercury could not in general be observed to have any inclination to perform the equatropical motions, although at times a very small tendency towards performing them might be perceived.

Tropical variation of the barometer.

On the 23d of August the barometers were taken from the shore to the ship. Immediately on leaving Bombay harbour, August 26th, 1802, the mercury in the barometers performed the equatropical motions, and continued them with great uniformity, during our passage down the Malabar coast, across the bay of Bengal, in the Strait of Malacca, and through the China Sea, until our arrival in Canton river on the 4th of October. When in the river, the mercury became nearly stationary during the 24 hours, except a small inclination at times towards the equatropical motions, but they were not near so perceptible as at sea; this change taking place the day we got into the river.

During our stay in China, the barometer on shore, at Canton, had very little tendency towards the equatropical motions, throughout the months of October and November that we remained there. At times, while in China, a small inclination towards performing the equatropical motions appeared: but, as in Bombay, the difference of rise and fall was of so small a quantity, as to be frequently imperceptible.

December 2d, 1802. On our departure from Canton river, the equatropical motions were instantly performed by the mercury, and with great regularity continued during the whole of the passage to Bombay, until our arrival in that harbour on the 11th of January, 1803.

On January 18th, the barometers were placed on shore, and did not appear in the smallest degree subject to the equatropical motions; although, with great regularity, they had been performed while at sea, even to the day we entered the harbour. One of the barometers was left on board for a few days, and, like that on shore, seemed to have no tendency towards the equatropical motions. During the months of February and March, in Bombay, the mercury was nearly stationary throughout the 24 hours. But about the latter part of March the mercury seemed to incline towards the equatropical mo-

Tropical variation of the barometer.

tions in a very small degree; and, during the month of April, and to the 20th of May, this small tendency of the mercury to perform the motions appeared at times, but was hardly discernible, the rise and fall being of so small a quantity. From the 18th of January to the 20th of May, the mercury in the barometers was in general stationary, except a very small tendency towards the equatropical motions at times. At other times some change in the atmosphere disturbed the mercury from its stationary position; but this was seldom the case, as it was then the fair weather season, or north-east monsoon.

We sailed from Bombay on the 23d of May, 1803. The instant we got out of the harbour, the mercury in the barometers conformed to the equatropical motions with great regularity, and the difference between the high and low stations was very considerable during the whole of the passage to China, excepting a few days in the eastern parts of Malacca Strait, where the land lay contiguous on each side of us; the difference between the high and low stations of the mercury was not then so great as in the open sea. On clearing the Strait, and entering the China Sea, the equatropical motions were performed in greater quantity, and continued regular during our passage up the China Sea, until July 2, 1803. We then entered Canton river, and the equatropical motions of the mercury in barometers entirely ceased.

From July 8th to September 7th, the barometers were placed on shore in Canton, during which time the mercury appeared to have no tendency towards performing the equatropical motions; but it inclined to a stationary position, except when influenced by changes of weather. After the barometers were taken from Canton to the ship, we were four days in getting clear of the river, in which time the mercury inclined to be stationary, excepting that a small inclination towards the equatropical motions seemed to evince itself at times. But no sooner had we cleared Canton river, September 13, 1803, than the mercury in the barometers began to conform to the equatropical motions, of two elevations and two depressions every 24 hours, at equal intervals of time, (although we were near the land until the 15th September.) And the mercury, with great regularity, continued to perform

form the equatropical motions, from September 13, 1803, the day we cleared the river of Cantón, until October 13, when we entered Singapore Strait, excepting a small degree of irregularity, which affected the mercury on the 22d September, when it blew a gale on the coast of Iliompa.

Tropical variation of the barometer.

October 13, 1803. On entering the Strait of Singapore, which is about $3\frac{1}{2}$ leagues wide, the mercury in the barometers was then a little obstructed, and did not perform the equatropical motions, in the same quantity of rise and fall, as when we were in the China Sea. But on the following day, October 14, when we had passed the narrow part of the Strait, the mercury conformed to those motions with regularity until October 21, when we arrived in the harbour of Prince of Wales's Island; then a great retardation took place in the equatropical motions; for, during the time the ship remained in the harbour, from October 20 to November 5, 1803, the mercury in barometers seemed only in a small degree subject to them, the difference between the high and low stations of the mercury, being in general not more than half the quantity, that takes place in the open sea, or at a considerable distance from land. Where the ship lay at this time in the harbour, the land, on one side, was a full quarter of a mile distant, and on the other side about $1\frac{1}{2}$ mile.

On November 5, being clear of the harbour of Prince of Wales's Island, the equatropical motions were instantly performed by the mercury, in the usual quantity experienced at sea, which continued with uniformity until December 3. On this and the following day, the mercury fell considerably during our passage over the tails of the sands at the entrance of Hoogly river, in latitude $21^{\circ} 06' N$; and on December 5, the day of the moon's last quarter, a gale of wind commenced from N N E, with much lightning and rain in the night. During the latter part of this day, the mercury began to rise, and there soon followed a change of settled weather. When we were in the lower part of the river, the mercury appeared to conform in a small degree to the equatropical motions; but when well up the river, at Diamond Harbour, the mercury inclined to be nearly stationary during the 24 hours, as has formerly been observed to happen in Canton river, Bombay harbour, &c.

Tropical variation of the barometer.

On January 13, 1804, after we had cleared^d the river Hoogly, the mercury in the barometers began to perform its motions with uniformity, which continued during the passage to Bombay, until our arrival there on February 12. The barometers being then placed on shore, the mercury inclined to a stationary position, without evincing any propensity towards the equatorial motions from the 12th to the 18th February, 1804, as has been noticed in the foregoing description, to happen frequently, on entering a harbour from sea.

On February 18, 1804, the meteorological journal ceases, at which time it comprises the observations of 22 months, having commenced April 6, 1802, in Margate Road.

I have taken the liberty of sending you this abstract from the journal, to exhibit the apparent difference of the mercury in the barometer at sea, from what has been observed on shore, at those places mentioned in the preceding description. As I have not seen any account indicating the phenomenon, I thought it might be interesting to you, or other gentlemen of the Royal Society to forward this imperfect abstract, the journal itself being too cumbersome to send home at present. But as I am in expectation of returning to England by the ships from China next season, I hope I shall be enabled to present you with the meteorological sheets alluded to.

I am, &c.

J. HORSBURGH.

P. S. Since I wrote the foregoing abstract, I have received a letter from my friend Mr. Dalrymple, intimating that a copy of the meteorological journal itself would be acceptable, which has induced me to transmit to him the original sheets, with a request to deliver them to you. I regret that I could not find leisure time to make out a fair copy, to have sent to you, in place of the original sheets in their rough state.

Bombay, June 1, 1804.

V.

Second Communication on Artificial Tan. By CHARLES HATCHETT, Esq. *Abridged from the Philosophical Transactions for 1805.*

§ I.

THE artificial tan*, procured as described in the first communication (*see our Vol. XII. p. 327*), had been named tannin by Mr. Hatchett; but the objection having been made to this, that tannin was destroyed by the nitric acid, while the artificial tanning substance was actually formed by it, induced Mr. Hatchett to expunge the word *tannin* wherever it had been applied to the latter. It also induced the author to make the following experiments on the comparative effects produced by nitric acid on those substances which contain most tannin, and also some others in which a tanning substance has been produced, under circumstances in some respects different from those described.

§ II.

Although it is not absolutely asserted that the tanning substance is indestructible by nitric acid, yet the following experiments prove, that to produce this effect must at least be the work of much time and difficulty.

1. Twenty grains of the artificial tan were dissolved in half an ounce of strong nitric acid, of the degree of 1.40; the solution distilled till the whole of the acid came over, which acid was returned back on the residuum, and the distillation repeated three times in this manner. Care was taken not to overheat the residuum; and then, when examined, did not appear to have suffered any alteration in its properties.

Name of the artificial tanning substance altered.

Experiments to prove that the artificial tan is nearly indestructible by nitric acid.

* In several parts of the abridgement of Mr. Hatchett's papers, the artificial tanning substance has been called *the new tan* and *artificial tan*, and tanning matter *tan*, for the sake of brevity. It was thought necessary to mention this, as the name tan is usually appropriated to oak bark in a certain state; which, with singular impropriety, is that in which it contains least tanning matter, after having been used in the tanners' pits.—ARR.

Experiments
continued,

2. Ten grains of the new tan, mixed with ten grains of white sugar, dissolved in half an ounce of nitric acid, was distilled to dryness. The residuum was not changed by the gelatinous or other re-agents.

3. This experiment was the same as the former, only that gum arabic was employed in place of sugar. The result was the same.

4. The precipitate from a solution of isinglass, with which the artificial tan had been mixed, was well washed with distilled water and then dried. In this state it was digested in strong nitric acid, by which a dark-brown solution was formed; which was evaporated to dryness, and the substance, dissolved in boiling distilled water, was examined by nitrate of iron, acetite of lead, muriate of tin, and solution of isinglass, with all of which it threw down copious precipitates, similar in all respects to the artificial tan, which had not been subjected to the process described.

5. Some of the precipitate of isinglass by the new tan was dissolved in muriatic acid, and evaporated to dryness: of this boiling distilled water dissolved only a part; and the solution, of a dark beer colour, did not precipitate gelatine, though it acted on muriate of tin and sulphate of iron; for with the former it gave an ash-coloured precipitate, and with the latter a slight deposit of a reddish-brown.

6. As boiling water dissolved only a part of the isinglass precipitate in the former experiment, the remainder was treated with nitric acid; after which, on being evaporated to dryness, it was found to be completely soluble in water, and precipitated gelatine as copiously as at first.

7. Twenty grains of the new tan was dissolved in half an ounce of muriatic acid: The residuum, after evaporation to dryness, appeared in every respect unchanged.

The author here makes the observation, mentioned at the conclusion of the former paper, relative to the solutions of the new tan not becoming mouldy like those of galls, sumach, and catechu, and seeming to be completely imputrescible.

And having thus ascertained the unchangeable nature of this substance, he made the following comparative experiments on galls, sumach, Pegu cutch, kascatti, common cutch, and oak bark.

8. Twenty

8. Twenty grains of powdered galls were dissolved in half an ounce of strong nitric acid: The residuum from this solution evaporated to dryness, and then dissolved in boiling water, did not produce the smallest effect on dissolved gelatine. Experiments on galls, sumach, catechu, kascutti, Pegu catechu, and oak bark.

The experiments on to No. 13. did not produce any tannin.

9. The residuum of a strong infusion of galls, treated as No. 8.

10. Ifinglafs precipitated by infusion of galls, dissolved in strong nitric acid, and examined as No. 4.

11. Twenty grains of sumach dissolved in half an ounce of strong nitric acid, and treated as No. 8.

12. Twenty grains of Pegu catechu (which contains much mucilage) subjected to a similar process, by which much oxalic acid was obtained.

13. Twenty grains of catechu, called kascutti, treated similarly, had, together with the four foregoing experiments, all the same results as No. 8, not any of them shewing any tannin.

14. Twenty grains of common catechu, dissolved in strong nitric acid, evaporated to dryness, dissolved in water, and examined by ifinglafs, deposited a tenacious film insoluble in boiling water, evidently composed of gelatine and tannin.

15. Twenty grains of oak bark treated in the same way, deposited also an insoluble film on the sides and bottom of the vessel.

16. Infusions of galls, sumach, and oak wood, of equal strength, were mixed with nitric acid, in the proportion of half an ounce measure of each to one drachm of the acid, and did not then render ifinglafs solution turbid.

But infusions prepared from oak bark and the artificial tan, and managed in the same way, continued to precipitate the gelatine, until four drachms of the nitric acid had been added to each half ounce of the infusion.

These results shew that artificial tan is the most indestructible, but that the other tanning substances have considerable varieties in this respect. The tannin of oak bark resists nitric acid longer than that of galls, sumach, kascutti, or Pegu catechu. This last is replete with mucilage, and yields much oxalic acid, as before described: it seems also to be the most destructible of all the kinds of catechu: From these facts the author was induced to add the sugar and gum to the artificial

tan,

tan, to promote its destrucibility; and expresses his belief that mucilage or gum renders the substances that contain it more destrucible in the nitric acid, and in some cases also prevents or impedes the formation of the tanning substance; which difference he thinks to be caused by the mucilage being in a state of chemical combination in those bodies.

§ III.

Experiments on
the artificial tan.

A and B. When sulphuric or muriatic acid was added to a solution of the new tan, it became turbid and deposited a brown precipitate, which was soluble in boiling water, and was then capable of precipitating gelatine; in which particulars it resembles the tannin of galls and other vegetable substances.

C. Carbonate of potash, added to a solution of the new tan, deepened the colour; the liquor became turbid, and deposited a brown magma.

D. Five grains of dried artificial tan were dissolved in half an ounce of strong ammonia: the whole was then evaporated to dryness; and being dissolved in water was found not to precipitate gelatin, unless a small portion of muriatic acid was previously added.

E. Another portion dissolved in ammonia was distilled: At first ammonia came over, and afterwards a yellow liquor, that had the odour of burned horn. The residuum was insoluble in water, to which it only gave a slight yellow tinge.

On distillation it
has an odour of
burned horn,
and yields am-
monia.

F. The object of this experiment is to shew the strange property of the new tan, of giving products analogous to animal matter (of which it yielded the odour in combustion on former trials), though prepared itself from vegetable substances. Some prepared from dry vegetable charcoal was distilled: First a little water came over, then a little nitric acid, then a very small portion of a yellowish liquor: The fire being then raised, the vessels suddenly became filled with a white cloud, and so great a torrent of gas was almost explosively produced as to upset the jar: This gas, by its smell, appeared to be ammonia, and was formed into the cloud by the nitric acid vapour in the vessels. The next jar of gas, which came slowly over, was carbonic acid, except a very small part which seemed nitrogen gas. A bulky coal remained, that on incineration gave $1\frac{1}{2}$ grains ashes, which consisted principally of lime.

G. Fifty grains of this substance were dissolved in four ounces of water and precipitated by isinglass solution; eighty-one grains of which became thus combined with forty-six of the new tan. The remaining portion was not precipitated, and was therefore separated on a filter and evaporated to dryness. It was a light brittle substance of a pale cinnamon colour, which, though composed of inodorous substances, had however a strong smell itself of oak bark; which is remarked as a singular circumstance; and this smell became stronger when the substance was put into water, in which it instantly dissolved.

The solution was very bitter; acted but slightly on dissolved isinglass; produced a brown precipitate with sulphate of iron, and with muriate of tin a black one; had no effect with nitrate of lime; but with acetite of lime gave a copious precipitate, of a pale brown colour. This substance appeared to be the tanning matter in the state of extract.

§ IV.

Several unsuccessful attempts were made to form the tanning matter by oxy-muriatic acid. It therefore appeared, that though a variety of it could be produced by the action of sulphuric acid on resinous substances, yet nitric acid was the most effective agent.

Attempts to form tanning matter by oxy-muriatic acid unsuccessful.

The author suspecting that the new tan might be formed from bodies not absolutely converted into coal, and not being able to get any touch-wood, which he first thought of trying for this purpose, made the following experiment with indigo, which he knew to contain much carbon.

Artificial tan, it is suspected, might be formed from substances not charred.

One hundred grains of indigo, with one ounce of nitric acid diluted with a double quantity of water, was (when the effervescence had subsided), placed in a sand-bath for several days till evaporated to dryness.

Experiments on indigo with this view.

The residuum, of an orange colour, was in great part dissolved by three ounces of distilled water poured on it, and gave a solution of a deep yellow, and intensely bitter; which, with the sulphate of iron, deposited a slight pale-yellow precipitate, and with nitrate of lime, a small white precipitate, having the character of oxalate of lime: With muriate of tin a copious white precipitate, that changed to a yellowish-brown;

brown;

brown; and with acetite of lead a beautiful deep lemon-coloured precipitate, which may probably prove useful as a pigment.

Ammonia rendered the colour much deeper, and with it deposited a large quantity of fine yellow spiculated crystals, which did not precipitate lime from its solutions. Their flavour was very bitter.

It produces tanning matter.

Lastly, when this solution was added to dissolved isinglass, it became turbid, and deposited a tough elastic insoluble film, and possessed the characters of gelatin combined with tanning matter.

Almost all vegetable bodies yield tanning matter, when subjected to repeated distillations with nitric acid.

By this experiment the possibility of producing tanning matter from bodies not converted into coal was fully ascertained; and the author has since discovered that though indigo yields this matter more readily than most other vegetable bodies, yet almost all produce it when subjected to repeated distillations with nitric acid.

Resin yields it by this treatment.

A. The common resin did not produce the tanning substance with nitric acid, but by the aid of sulphuric acid, as before related; yet upon this nitric acid being repeatedly abstracted from it, its solution in water formed a tough yellow insoluble precipitate with dissolved gelatin, similar to that by solution of indigo, and with other re-agents produced the following effects.

With sulphate of iron, after 12 hours, it produced a slight yellow precipitate. With nitrate of lime no effect. With muriate of tin, after 12 hours, a pale brown precipitate. And with acetite of lead a very abundant precipitate of a yellowish white colour.

On repeating this experiment, the author remarked that during each distillation nitrous gas was produced, while the acid which came over was weakened, which made the cause of the change in the properties of the resin evident. The following are the results of experiments tried with other resinous substances.

As do likewise thick lac,

B. Stick lac, treated as described, copiously precipitated gelatin.

—and balsam of Peru,

C. Balsam of Peru during the process afforded some benzoic acid, and gelatin was precipitated by the aqueous solution.

Benzoin, after the sublimation of some benzoic acid, yielded ^{—and benzoin,} a residuum, which yielded with water a pale yellow solution, of a very bitter flavour.

This solution with sulphate of iron produced a slight pale yellow precipitate. With nitrate of lime, no effect. With muriate of tin in solution, a small quantity of brownish white precipitate. With acetite of lead, a copious pale yellow precipitate. And with solution of isinglass, a dense pale yellow insoluble precipitate.

E. Balsam of Tolu afforded benzoic acid, and the solution of the residuum precipitated that of gelaten.

F. One hundred grains of dragon's blood in powder mixed ^{—and dragon blood,} with one ounce of nitric acid, evolved much gas; an ounce of water was then added; and the digestion in a sand bath being continued, after it produced chafication on the dry yellow mass that remained, a brilliant feather-like sublimate arose, which weighed rather more than six grains and had the aspect, odour, and properties of benzoic acid.

The residuum, of a brown colour, formed with water a gold coloured solution, which was not affected by nitrate of lime: But with sulphate of iron, and with muriate of tin it formed brownish yellow precipitates; and with acetite of lead one of a lemon colour.

Gold was precipitated by it in the metallic state, and the containing glass coloured purple; and with dissolved isinglass it produced a deep yellow insoluble deposit.

As dragon's blood simply exposed to heat did not produce any benzoic acid, the author is inclined to believe, that in the first experiment this acid was obtained as a product, and not as an educt.

G. Gum ammonia gave a brownish yellow, bitter astringent solution; which with sulphate of iron became of a darker <sup>—and gum am-
monia,</sup> colour, but produced no precipitate.

With nitrate of lime, a slight precipitate. With muriate of tin and acetite of lead, copious yellow precipitates; and with gelaten a bright yellow insoluble deposit.

H. Assa fœtida yielded a solution which precipitated gelaten <sup>—and assa fœ-
tida.</sup> in a similar manner to that described.

I. Solutions of elemi, tacamahac, olibarum, fundarach, <sup>Solutions of
elemi, tacama-
hac, and did not
though affect gela-</sup> copaiba, massich, myrrh, gambage, and cacutchone, al-

though they precipitated the metallic solutions, did not affect gelatin; but possibly might have done so, if the process had been more frequently repeated.

—not that of
sarcocol,
—nor of gum
Arabic,
—nor of traga-
anth,
—nor of manna.

K. Sarcocol also produced similar results.

L. Gum Arabic afforded oxalic acid but no tan.

M. Tragacanth yielded much of saccharic acid, of oxalic, and of malic acid, but not the least tan.

N. Manna gave oxalic acid, part of which sublimed in the neck of the vessel.

Its residuum formed a brown solution, which produced precipitates of the following colours: With sulphate of iron, a pale yellow; with muriate of tin, a pale brown; with acetite of lead, a brownish white. From nitrate of lime, oxalate of lime was copiously precipitated by it; but with isinglass solution no effect was produced.

Liquorice solu-
tion precipitates
gelatin.

O. Nitric solution of liquorice yielded precipitates with sulphate of iron and muriate of tin, after twelve hours, slight brown. With acetite of lead, a brownish red. With nitrate of lead, a brown. And with gelatin, one of a yellowish brown, insoluble, and similar to other precipitates from it by tan.

Guaiacum solu-
tion gave a slight
precipitate with
gelatin, which
was soluble in
water.

P. On guaiacum nitric acid acted with great vehemence and speedily dissolved it: The residuum was almost totally soluble in water; and this solution produced effects on the metallic salts similar to those recited; but with gelatin formed a slight precipitate, which was speedily dissolved by boiling water. The remainder of the solution evaporated gave a large quantity of crystallized oxalic acid; so that in this respect guaiacum was similar to the gums, and unlike the resins.

§ V.

Experiments on
several roasted
vegetable sub-
stances, which
do not affect
gelatin.

As many vegetable substances when roasted yield a liquor by decoction, resembling solution of artificial tan, the author tried those similarly prepared, of dried peas, horse beans, barley, and wheat flour, none of which gave any precipitate with gelatin.

Coffee gives a
precipitate with
it, which is again
soluble in water.

The decoction of coffee also gave no precipitate till after several hours, and then one soluble in boiling water; but this might be occasioned, the author thinks, from want of some particular nicety which may be required in passing such bodies

so as to make them yield tan; which opinion was corroborated by experiments made by decoction of chicoree (probably endive) root, prepared in the same manner, which produced a precipitate with gelatin after some time, though not at first, which was apparently dissolved in boiling water, but deposited again in its original state, on cooling. The author therefore is inclined to believe that the tanning substance is really developed in many vegetable matters by heat alone; but that a certain degree of heat, not easy to determine is absolutely necessary for this effect.

A small quantity of nitric acid added to any of the decoctions just mentioned, and evaporated to dryness, produced a residuum, having all the properties of the tan produced from coal.

Nitric acid added to these decoctions gives tanning properties to their residue.

§ VI.

The production of a variety of the tanning substance before mentioned, by the action of sulphuric acid on the resins, amber, &c. suggested the following experiments on camphor; the results of which tend to increase the knowledge of its properties.

Experiments on Camphor with Sulphuric Acid.

The only facts hitherto related relative to the effects of sulphuric acid on camphor, are that a brown or reddish brown solution is formed, from which water precipitates the camphor unchanged; but this only happens at a certain period of the operation; for if it be longer continued, the following effects will be produced.

A. One ounce of concentrated sulphuric acid was added to one hundred grains of camphor, which dissolved gradually, after first becoming yellow; in about an hour, the liquor having progressively changed to reddish brown, brown, and at last blackish brown, much sulphureous acid gas was produced, and continued to encrease during four hours, when the whole appeared a thick black liquid, having no other odour but that of sulphureous acid; after two days the production of the gas was much diminished; the containing alembic was then put in a sand bath, moderately hot, by which more sulphureous gas was obtained; but this soon abated; at the end of two days more, six ounces of water was gradually added, by which the liquor

Experiments with sulphuric acid on camphor.

An odour
yielded by it
like oils of l-
vender and pep-
permint.

liquor changed to a reddish-brown, a coagulum of the same colour subsided, the odour of sulphureous acid gas was immediately annulled, and was succeeded by one which much resembled a mixture of oils of lavender and peppermint.

The whole was then distilled gradually; when the water came over impregnated with the odour last mentioned, accompanied by a yellowish oil, which floated on the top, and was computed to amount to three grains.

B. When the whole of the water had come over, there was again a slight production of the sulphureous acid gas; two ounces of water were then added, and the distillation continued (without the recurrence of the former odour) till a dry blackish brown mass remained; this mass was well washed with warm distilled water, by which nothing was extracted; but two ounces of alcohol digested on it for 24 hours formed a very dark brown tincture.

The residuum was digested with two ounces more alcohol, and the process repeated till the alcohol ceased to act.

The residuum had now the appearance of a compact sort of coal in small fragments, which were well dried, and after being exposed to a low heat in a close vessel, weighed fifty-three grains.

C. From different portions of the alcohol solution, added together and distilled in a water bath, a blackish brown substance was obtained, which had the appearance of a resin or gum with a slight odour of caramel, and weighed 49 grains.

The products obtained from the 100 grains of camphor treated with sulphuric acid, were,—

	Grains.
A. An essential oil, having somewhat of an odour of a mixture of lavender and peppermint, about	3
B. A compact and very hard sort of coal, in small fragments,	53
C. A blackish-brown substance, of a resinous appearance,	49
	<hr/> 105

Products from
camphor and sul-
phuric acid.

The increase of weight of five grains is attributed partly to water retained by the last substance, and partly to oxygen united to the carbon.

The substance C had the following properties :

1. It

1. It was bitter and astringent, had the odour of caromél, and formed with water a dark-brown solution.

2. This solution produced very dark-brown precipitates with sulphate of iron, acetite of lead, muriate of tin, and nitrate of lime.

3. Gold was precipitated by it in the metallic state from its solution.

4. By solution of isinglass the whole was precipitated; so that after four hours a colourless water only remained.

Substance C
(from camphor)
precipitates gelatin.

This precipitate was nearly black, and was insoluble in boiling water: from whence, and its effects on skin, it was evidently a variety of tanning matter much resembling that obtained from resinous bodies by sulphuric acid.

But this sort of tan had less effect on skin than that procured from carbonaceous substances by nitric acid, and its precipitate from gelatin was more flocculent and less tenacious.

However, when a small quantity of nitric acid was added to the solution of the substance obtained from camphor, and when the residuum, after evaporation to dryness, was dissolved in water, a reddish-brown liquor was formed, which acted in every respect similar to the tanning substance obtained from the varieties of coal by the nitric acid.

§ VII.

From the experiments related, it appears that three varieties of the tanning substance may be formed.

The three varieties of artificial tan.

1st. That produced by nitric acid with any carbonaceous substance, whether vegetable, animal, or mineral.

2d. That formed by distilling nitric acid from common resin, indigo, dragon's blood, and various other substances.

3d. That which common resin, elemi, assafœtida, camphor, &c. yield to alcohol, after they have been previously digested with sulphuric acid.

On these products the author makes the following remarks:

Remarks on them.

The first variety is the most easily formed. From some experiments made purposely it appears, that, after making allowance for a small quantity of moisture and of nitric acid remaining, 100 grains of vegetable charcoal yield 116 of the dry tanning substance.

100 grains charcoal
yield 116
tanning matter.

Carbon the base of the tanning matter.

From the manner in which it is produced, carbon is evidently the base and predominating essential ingredient in this substance.

It also contains oxygen, hydrogen, and nitrogen.

From § III. experiment F, it also appears that the other component parts are oxygen, hydrogen, and nitrogen; for when the artificial tan was distilled, ammonia and carbonic acid were obtained, exclusive of a small portion of a yellow liquor that appeared to be of an oily nature, from being insoluble in water and alcohol.

It has an odour like animal matter when burned, and one of oak bark when precipitated as in G, § III.

Many of the properties of the tanning substance prepared from coal by nitric acid are very remarkable, particularly those noticed in § III. experiment F; of its having the odour of animal substances when burned, though prepared from vegetable matter; and in experiment G, of the precipitate having the odour of oak bark, though the component materials were inodorous.

It resembles vegetable tannin in most properties.

But its most extraordinary properties are those in which it so nearly approaches the vegetable *tannin*, which it perfectly resembles in its solubility in water and in alcohol, in its action on gelatin and on skin, in its effects on the metallic solutions, on the alkalis, and on the earths.

Difference between it and tannin.

The sulphuric and muriatic acids also affect its solutions, as they do those of tannin; and the only marked difference between artificial tan and tannin is, that the former is produced by nitric acid, while the varieties of the latter are more or less destroyed by it; but here it must be remembered, that even the varieties of tannin do not accord in the degree of destructibility.

Second variety of artificial tan.

The second species of the tanning substance is obtained from a variety of vegetable bodies before recited, by digesting and distilling them with nitric acid. It is therefore not so readily prepared, and the quantity of it produced is less in proportion to the substance from which it is prepared.

Theory of its formation.

As resin and some other bodies do not afford it until they have been repeatedly treated with nitric acid, and as, during each operation, nitrous gas is produced, while the strength of the acid which comes over is diminished, the author thinks it almost certain that the tanning substance is formed in consequence of part of the oxygen of the nitric acid becoming combined with the hydrogen of the original body, so as to form

water; and the carbon being thus in some measure denuded, is rendered capable of being acted on gradually by the nitric acid, in a manner nearly similar to what takes place when it has been previously converted into coal.

The precipitates of this tanning substance from gelatin are always pale or deep yellow, while those formed by the first species are constantly brown; which induces the author to believe that the different colours of the precipitates depend on the state of the carbon of the tannin.

The quantity of artificial tan obtained from resin and other bodies, was always less than that from coal, or even from the same bodies previously converted to coal in the humid way by sulphuric acid. The cause of this seems to be, that a number of other products are simultaneously formed with the tanning substance, all of which require more or less carbon as an ingredient; so that, according to the affinities which prevail, some bodies afford but little, and others none of it.

Quantity of artificial tan left from resins than from coal, accounted for.

The greatest proportion of this substance was yielded by indigo, common resin, and stick lac.

Its proportions from different substances.

The quantity obtained from assafœtida and gum ammoniac was less.

Benzoin, balsam of Tolu, balsam of Peru, and dragon's blood, were inferior to the former in this respect; so that the production of benzoic acid seemed to counteract the formation of the tanning substance. But oxalic acid, when formed in any considerable quantity, seemed absolutely to prevent the formation of this substance: for gum arabic, tragaçanth, manna, and guaiacum, which produced oxalic acid in abundance, yielded no tanning matter.

Benzoic acid when formed counteracts the formation of the tan; and oxalic acid, when produced, prevents it entirely.

Common liquorice seems to be an exception; but the author supposes that the small quantity of tan produced by it, was formed by the action of the nitric acid on a portion of uncombined carbon, which being in a state approaching to coal, is probably the cause of the blackness of the common liquorice.

The third variety of the tanning substance appears to be uniformly produced during a certain period of the process; but by a long continuance of the digestion there is reason to think it is destroyed.

Third variety of artificial tan.

Substances, such as gums, which yield much oxalic acid, do not apparently afford any of this tanning matter.

The energy of its action on gelatin and skin is certainly inferior to that of the first variety, into which however it may easily be converted by nitric acid.

From the mode of its formation, there does not appear to be any evidence of its containing nitrogen like the first and second varieties, and perhaps the absence of nitrogen may be the cause of its less powerful action.

Experiments of Messrs. Biggin, Proust, and Davy on tannin, noticed.

In the course of the communications on this subject, Mr. Hatchett notices the experiments on tannin by Mr. Biggin, the great contributions of M. Proust to the elucidation of its nature and properties, and the very great extension of, and valuable additions to the same, from the ingenious labours of Mr. Davy, particularly his discovery of the singular fact that terra japonica, or catechu, consists principally of tannin.

Medicine, arts, &c. may derive great benefit from farther investigations of gums, resins, &c.

The author also greatly recommends the farther investigation of the nature of the gums, resins, balsams, and gum-resins, by every possible method; and is of opinion, that medicine, arts, and manufactures may derive many advantages from it, and the mysterious processes of vegetation probably receive considerable elucidation.

VI.

On carbonised Turf. From a Report made to the Prefect of Police (at Paris) on the Methods employed for reducing it to this State. By MM. CALLIAS and Co.*

The use of turf very ancient. It produces no deleterious effects.

THE use of turf for domestic fuel is of a very ancient date: Some of the most eminent men of science have pronounced that it does not produce any deleterious effects. Without citing the examples of England (Ireland), Scotland, and Holland, where great quantities of it are consumed, we will confine ourselves to the use made of it in France, in the (*ci-devant*) provinces of Flanders, Artois, and Picardy.

Its use is now tolerated in Paris to relieve the scarcity of wood: the lime-burners, plaster-bakers, brick-makers, and washers, make great use of it both in the city and its vicinity; and it has never been perceived that those who lived within

* Sonini's Journal, Tom. II. p. 324.

the influence of its smoke, have experienced any bad effects from it. The commissioners (employed to make this report) observe, that the great volume of smoke which is disengaged on the commencement of its combustion, is only caused by a great portion of water contained by the turf, which is expanded into steam by the heat; soon afterwards this smoke is combined with an acid analogous to that of vegetable substances, which, far from making the air deleterious, tends on the contrary to neutralize the vapours of infection which it may contain. It is true that sometimes, for an instant, the turf in combustion exhaled an odour of empyreumatic oil, in the form of gaseous vapours, but this odour is by no means injurious to the animal organisation, but, on the contrary, is beneficial in nervous affections.

Its smoke is caused by contained water;

with an acid vapour, which removes infection.

But if this odour is disagreeable when the turf is burned in towns, villages, and private houses, this complaint cannot take place when it is burned in the open air at a distance from all habitations, which will be effected by its previous carbonification, as managed by MM. Callias and Co. therefore the company merit the public protection.

Charing prevents all the unpleasant effects of its odour.

In 1785 the French Government took a great interest in what related to the carbonification of turf, and granted 80,000 francs to a company to erect a furnace for this purpose on the ground of the Capuchins. The method of this company was that of extinguishing, but their plan did not succeed, and the works were abandoned.

Company at Paris for charring turf, assisted by the government, did not succeed.

A new company tried, some time after, the same enterprise, at its own expence; the method of operating in closed vessels was proposed: the experiments made were on a great scale, and were attended with a success that was certified by the commissioners of government: a memoir printed in 1790, by M. Morclot, contained these facts, with a statement of the superiority of turf-charcoal over that of wood. But the disastrous events of the revolution put an unhappy end to this enterprise which promised so well.

Another company—promised well—but the revolution caused its failure.

At present MM. Callias and Co. offer to the public an additional species of fuel to that hitherto in use, a charcoal of a new process, the materials of which are spread with profusion over the territory of France, and the consumption of which, being substituted for that of wood, will at the same time be an object of economy to individuals, and of incalculable advantage.

Callias and Co. charr turf by a new process.

The use of turf charcoal. It will be cheap.

and prevent the
destruction of
the forests.

vantage to the management of the forests. Timber for the construction of houses and furniture, and timber for ship-building, daily increase in price, because they become more scarce. Some of the forests have become reduced, as the fresh growths in them do not keep pace with the destructive instrument that overturns them; some of them are entirely destroyed, and the ground converted into ornamental gardens; and thus each year, each month, each day, conducts us insensibly to a most alarming dearth of timber. Already the price of fire-wood is tripled, and Paris is on the eve of being deprived of a combustible which, as yet, has not been replaced to advantage.

The carboniza-
tion used by
Callias is very
perfect.

The commissioners compliment MM. Callias and Co. whose method of carbonisation is peculiar to themselves, and calculated conformably to the laws of combustion in its two first stages; that is to say, before the arrival of its third degree, or that of absolute combustion. MM. Callias and Co. by their method, direct the carbonisation at their pleasure and in an invariable manner; they are always sure of obtaining a perfect charcoal, without smoking-pieces, and without any risk of forming it into a pyrophorus, which sometimes happens in the carbonisation performed in closed vessels. Their manner of proceeding is also very economical; and what proves that they work with intelligence is, that they daily improve, and already are able to save ten hours out of 48 in each carbonisation.

Experiments made with Charcoal of Turf.

Turf-charcoal
yields more heat
than wood-
charcoal.

1st. The charcoal of turf kindles a little slower than that of wood, but when it is once in complete ignition, it throws out much more heat; its flame is also more elevated, and it yields no odour, except a very slight one of sulphur, which ceases when it is fully lighted.

Causes water to
boil four times
as speedily.

2. Charcoal of turf, in equal quantity with charcoal of wood, caused a given quantity of water to boil four times; while that of wood caused it only to boil once. The first is then superior to the second in a quadruple proportion.

3. To prove that turf charcoal emits more heat than wood charcoal, the following experiment was made.

It fused 11 oz.
of gold in eight
minutes; wood-
charcoal did the
same in sixty.

With turf charcoal, in a goldsmith's furnace, eleven ounces of gold were fused in eight minutes, which with wood charcoal was not performed in less than sixteen minutes. The gold lost nothing of its malleability in the fusion with the turf; but,

on the contrary, it was necessary to add some reductive flux, to that fused by the wood charcoal, in order to restore the malleability which it had lost. minutes. The malleability of the gold was preserved.

4. Iron made red-hot by charcoal of turf in a forge, became more malleable; which proved that it gave none of its carbon to the metals with which it came in contact. Iron heated by it becomes more malleable. It lasts longer than wood-charcoal.

5. Finally, Turf charcoal lasts longer in a state of ignition than charcoal of wood, and its heat is constantly equal.

Conclusion.

1. The odour of turf in combustion is noways deleterious. Its odour not at all unwholesome. This truth has been confirmed by the most distinguished chemists; and is besides proved by the constant use made of this fuel in the *ci-devant* provinces of Flanders, Artois, and Picardy.

2. It is desirable that the carbonisation of turf may be encouraged, on account of the great advantages which may result from the use of this new species of charcoal, both for private consumption and for large works. Its use ought to be encouraged,

But the greatest matter in its favour is, that its use tends to diminish the felling of the forests, whose extension ought to be promoted by every means possible, and which nothing tends so much to destroy as the use of wood charcoal. and will preserve the woods.

VII.

Account of the Cataracts and Canal of Troellhætta, in Sweden, (from a Work relative to them by Colonel SKIOELDEBRAND. Published in one Volume Quarto, at Stockholm.)

THE cataracts of Troellhætta produce one of the finest effects which nature affords in Europe. The river of Gothie is the only outlet of the vast lake of Wener, navigable through its whole extent. This river, which falls in the North sea near Gothenberg, as soon as it departs from the lake, which is much more elevated than the sea, rolls its waters with impetuosity, and dashes them against steep rocks, whose resistance forms a succession of cataracts, which without being individually very high, form altogether a most striking object. The imagination is the more affected by this sight, as the surrounding scenes are of a dark and melancholy character, consisting of grey rocks, The cataracts are formed by the river Gothie near soon after its departure from lake Wener. They form a very striking object.
crowned

Ruins of locks which had been formerly constructed in the bed of the cataracts.

The canal passes by the side of the cataracts. Is partly cut in the rock. Its breadth, depth, number of locks, time of excavation, and cost. The extent of its navigation, and number of vessels which have passed.

crowned with ancient firs, and of frightful precipices, formed by the bursting of the locks and banks, which the fury of the water has overturned. These last were constructed in the bed of the cataracts, in order to render the river navigable through its whole length; but this daring work of man could not resist the reiterated efforts of nature, and therefore it was necessary to have recourse to another plan.

The canal newly constructed passes by the side of the cataracts, and its bed is partly formed in the natural rock, and partly in a marshy soil. It was began in 1794, and finished at the end of six years, in 1800. Its breadth is 22 feet, and its depth six feet and an half. Its locks are eight in number, and its cost amounted to the sum of 59833 pounds sterling, which was collected by subscription. By means of this canal there is a continued navigation, without any interruption, from the province of Wermeland to Gothenberg. In 1802 the number of vessels which had passed this canal amounted to 1350, which is at the rate of 1190 each year.

VIII.

Letter from H. B. K. on the Production of Nitrous Acid, and other Facts.*

To Mr. NICHOLSON.

SIR,

Experiments announced. Carbonate of potash in water was galvanized, and emitted carbonic gas.

AS Mr. Accum has not answered my paper, he therefore knows of no experiments which shew the formation of the nitrous acid; but anxiously impressed with the subject, I have been performing some experiments, which I think will throw great light upon the cause of the nitrous acid appearing in electrical experiments.

The potash became capable of effluvia like nitre; and solution of silver shewed the presence of muriatic acid.

I passed the galvanic fluid through a watery solution of the carbonate of potash, made by distilled water, confined in a glass tube where no atmospherical air could have access to it, and I found a great production of air come from the solution, which upon examination was pure carbonated air; and

* See our Journal, X. 105, 214, and XI. 105.

then examining the solution by dipping a piece of paper into it; upon its being dipped the paper shewed evident signs of nitre upon it, and when burned it detonated the same as nitre would have done; and also with the solution of silver the alkaline solution gave some faint indications of the marine acid being present in it. That the solution should give indications of possessing both the nitrous and marine acids is not so surprising; as we have the same products in firing oxygen and hydrogen gases, according to the foreign experiments, principally the nitrous acid, but with it a small quantity of the marine acid.

Remark. These two acids are found on the common detonation of oxygen and hydrogen.

I then filled the tube (after washing it clean) with pure distilled water, and sent through it the galvanic fluid; and I observed a generation of airs, which, upon examination, appeared to be the airs usually formed in these experiments, as they exploded.

Pure water galvanized gave these gases.

After this I filled the tube with a solution of pure potash in distilled water, and no air came from it upon galvanizing it; if any, it was carbonated air: But upon examining the solution, it gave clearly the same indications of possessing the nitrous and marine acids, as the carbonated solution of potash did in the first experiment.

Pure potash and water gave no gas by galvanizing, but indicated the two acids of the first experiment.

Now exempt from all hypotheses, let us examine these interesting facts: The carbonated potash had its carbonic acid air expelled, clearly from the acid or acids; as we know that it could not part with its carbonic acid air, but from the action of a stronger acid. Also another more essential fact it proves to us, that the generation of the peculiar airs, what are called oxygen and hydrogen gases, are owing to the acids; for when the potash was in the water as to arrest or attract them, there were neither of these airs produced; and upon examining the distilled water (in the experiment in which they were produced) after their production, there was no acid in the water, but it was pure distilled water. Therefore, beyond a doubt, the nitrous acid is essentially concerned in the production of these peculiar oxygen and hydrogen gases: indeed Mr. Cruickshanks says, that upon fusing these airs, he found in the residuum the nitrous acid.

Remarks. The carbonic acid was expelled from potash by a stronger acid produced. Whence it is inferred that the oxygen and hydrogen arose from these acids.

These experiments were performed by two short gold wires attached to each end of the galvanic pile, But upon placing a pretty long iron wire to the silver end of the pile instead of the

The conducting wires were gold.

the gold one, a little hydrogen gas was produced; even when the potash was mixed with the distilled water, though there was none when it was a gold wire.

I hope Mr. Nicholson you will not refuse the insertion of these interesting facts in your Journal, for I have made the relation of them as brief as possible that they might not occupy too much room*.

H. B. K.

London, August 15.

IX.

Report of M. DEBUC's Memoir on Acetic Acid, made by M. M. PLANCHE and BOULLAY, by Order of the Society of Pharmacy at Paris †.

M. Debuc repeated M. Badollier's process to obtain acetic acid from acetate of lead by sulphate of copper.

Debuc saw in the *Annales de Chimie*, No. 109, a method of M. Badollier, apothecary at Chartres, for obtaining acetic acid very readily from a mixture of equal parts of sulphate of copper and acetate of lead by a moderate heat.

The product used in manufacture produces an effect contrary to the common acetic acid.

The process again repeated, and its results carefully examined.

Relying on this process M. Debuc made an exact mixture of two pounds of sulphate of copper and an equal quantity of acetate of lead, which he exposed in a distilling apparatus on a sand bath to a moderate fire, which he increased by degrees during the operation, which lasted for six hours: the product obtained was 26 ounces. It was given to a manufacturer, without examination, and being used in his business produced an effect entirely contrary to the acid extracted from crystals of copper.

This circumstance determined M. Debuc to repeat the process as before, and examine its results carefully: In which he observed the mixture of the two salts to become pasty, which is easily explained by the difference of the concentration of the acid in the sulphate of copper, from that in the sulphate of lead. The products of this experiment were,

* I am extremely sorry that this communication was by mistake placed among papers already printed; which alone has caused the delay in its appearance.—W. N.

† *Annales de Chimie*, Tom. LIV. p. 145.

1. Four ounces of water slightly acidulated.

2. Four ounces of a liquor more acid than the first, and which

Products of this process.

M. Debuc compares to good vinegar of *Saumur*.

3. Eighteen ounces of a very limpid liquor, with a lively and penetrating odour of acetic acid mixed with sulphurous acid.

The residue, weighing 38 ounces, appeared to M. Debuc in different layers more or less red, according to their distance from the bottom of the retort; and he found the upper part covered with a whiteish powder, slightly inclined to a citron colour, in which he recognised the presence of sulphur.

The residue is in layers of different colours.

Parvies, the muriate of lime, and the acetate of lead formed immediately considerable precipitates with the third product.

Precipitates formed with the third product by different salts.

M. Debuc observes that the decomposition of the acetate of lead by the sulphate of copper may be easily explained; but that here there is a production of sulphurous acid, and a decomposition of the sulphuric acid from the absorption of its oxygen by the vinegar; which is a singular phenomenon, that has no agreement with the affinities of the acidifying principle for the acidifiable and fusible bases; he leaves the explanation of this matter to more experienced chemists, and only notices that the transportation of the oxygen of the sulphuric acid to another base, suggests the idea, that acetic acid is superoxygenated vinegar.

Sulphurous acid being produced, induces M. Debuc to suppose that acetic acid is super-oxygenated vinegar.

M. Debuc succeeded in freeing his third product from the sulphurous and sulphuric acids, by letting it remain for about 24 hours, on twelve grains of salt of tartar, and about two ounces of black oxide of manganese pounded fine, and after that distilling it slowly; by this rectification he obtained a pound of pure acetic acid of a lively and agreeable odour, and of about 10 degrees specific gravity; which is one degree less than that of radical vinegar well rectified, obtained from acetate of copper.

M. Debuc's method of freeing the third product from sulphurous and sulphuric acids. Acetic acid produced, one degree weaker than the common kind.

The author concludes from this,

1. That the product of two pounds of acetate of lead, treated with an equal quantity of sulphate of copper, is twenty-six ounces; of which four ounces is acidulated water, an equal portion strong vinegar, and eighteen ounces acetic acid altered by the sulphurous and sulphuric acids.

2. That the eighteen ounces, forming the third product, M. Debuc rectified as recited, does not differ from that drawn from crystals of acetate of copper, but by its less density.

cludes that the acetic acid produced only dif-

3. That

fers in strength from the common kind, and may be substituted for oximuriatic acid, in some cases, to advantage.

The reporters repeat M. Badollier's process, with M. Debus's modifications.

3. That in many cases this acid may be substituted for oximuriatic acid, as an object of salubriety without possessing its inconveniencies.

The reporters repeated the process of M. Badollier with the modifications advised by M. Debus as follows.

They introduced a mixture of two pounds of sulphate of copper, and the same quantity of acetate of lead into a glass retort, placed it on a sand bath, and adjusted to it a tubulated receiver, which communicated with two bottles of a Wolf's apparatus; the first of which contained distilled water, and the second many pounds of lime water; from this last a tube was passed underneath a jar in an hydro-pneumatic apparatus: the retort was heated gradually to the end of the operation, which lasted more than 10 hours; and the following products were drawn from the receiver.

Products of their process from the receiver.

1. Eight ounces of a liquor similar to distilled vinegar, but with a less agreeable odour.

2. Ten ounces of a liquor with an unpleasant odour of acetic acid, more penetrating than the first, and not containing any trace of sulphurous or sulphuric acids.

3. Finally seven ounces of a liquor of great limpidity, with a very pungent odour of sulphurous acetic acid, and which did not precipitate muriate of barytes.

A considerable disengagement of an elastic fluid was observed, which became perceptible as soon as the retort began to run, and which lasted during the whole operation.

Carbonic acid gas evolved.

This gaseous fluid was absorbed almost totally by the lime water, forming with it a very abundant white precipitate; which, gathered on a filter, and dried, proved to be carbonate of lime: It weighed two hundred and fifty grains, which made the carbonic acid equal, according to the known proportions of this substance, to eighty-five grains; atmospheric acid alone passed under the jar mixed with some carbonic acid gas: no trace was perceived of hydrogen gas.

Many layers of different colours were found in the retort.

The first was of a beautiful green, surrounded with a circle of yellowish white towards the sides.

The second, much more thick, was of a red colour, greatly like copper in very small particles.

The third was a mixture of sulphate of lead and of copper apparently in the metallic state.

The

The last larger, which occupied the bottom, of a black colour, and shining, was a mixture of sulphate of lead and of charcoal. The lowest layer of the residue is a mixture of sulphate of lead and charcoal.

The same experiment with the same quantities of the salts, was repeated a second time, with the precaution of reducing the sulphate of copper by disiccation to $\frac{1}{12}$ of its weight. The product from this was preferable to the other. The same process is again repeated; drying the sulphate of copper, the produce is better.

The second and third products were mixed and rectified on carbonate of potash and oxide of manganese, with the precautions indicated by M. Debuc: This rectification produced an acetic acid of nearly the same specific gravity as that afforded by simple distillation from crystals of copper, but of a less strong odour, less agreeable, and besides mingled with sulphurous acid. Second and third products rectified. A weaker and less agreeable acetic acid produced, mingled with sulphurous acid.

The reporters think that M. Debuc is deceived in his theory, "*that acetic acid is vinegar super-oxygenated by the oxygen of the sulphuric acid passing to the vegetable acid,*" for he has not considered,

1. That the acetic acid is almost all obtained, before the sulphurous acid becomes perceptible. Reasons why M. Debuc's theory is erroneous.
2. That the metallic oxides, which are the basis of the salts employed, have less attraction than sulphur to oxygen.
3. That the disengagement of the carbonic acid is much more likely to explain the matter.

The considerable production of carbonic acid, and the presence of charcoal in the residue, surprised the reporters the more; as MM. Boddolier and Darac (the first in his notice of the preparation of acetic acid; the other in a memoir, in other respects very interesting, on the difference of acetous and acetic acids,) positively assert that in the operation related, *there was no other gaseous production but that of part of the air contained in the vessels, especially no carbonic acid, and not an atom of charcoal in the residue.* The production of carbonic acid and charcoal from the process, is contrary to the assertions of MM. Boddolier and Darac.

The result found by the reporters so different from that of M. Darac, in an experiment on which he supports his theory of the identity of the acetous and acetic acids, was so favourable to the theory of M. Chaptal, that they would have been induced to decide in favour of the opinion of the latter, if the following comparative experiments had not confirmed them in a contrary notion, and appeared to them one of those, of which M. Darac might most avail himself.

An experiment made, favourable to M. Darac's opinion.

To four ounces of pure concentrated radical vinegar (extracted from crystals of copper by heat alone) were added, by degrees four ounces of semi-vitreous oxide of lead (litharge) in powder; which completely dissolved in it by heat, there even remained an excess of acid, perceptible in the strong odour of the solution. Being laid by to cool, it produced a very irregular crystalline mass.

Four ounces of this mass of acetic lead, mixed with an equal quantity of sulphate of copper dried, were treated in a convenient apparatus. The acetic acid produced had an odour more penetrating and agreeable: but all the other phenomena were the same as with the acetate of lead; that is to say, there was an equal developement of carbonic and sulphurous acids, and charcoal was found in the residue.

Which determined the reporters to conclude,

The reporters conclude that this acetic acid is always mixed with sulphurous acid; of which it cannot be freed entirely by M. Debuc's process; and never has so pleasing an odour as the common kind.

1. That acetic acid formed by the distillation of a mixture of sulphate of copper and acetate of lead, is always mixed with sulphurous acid, which does not become perceptible till towards the end of the distillation.

2. That it cannot be completely deprived of this sulphurous acid by the rectification proposed by M. Debuc.

3. That the acid itself, totally deprived of the sulphurous acid, is never of so lively and agreeable an odour, as that drawn from the crystals of the acetate of copper.

4. That it is preferable to dry the sulphate of copper before it is used.

5. That MM. Boddolier and Darac, were mistaken in supposing, that no carbonic acid was obtained in this operation.

6. Finally that the production of carbonic acid does not any more prove the decarbonisation of the acetous acid, it becoming the acetic, than the sulphurous acid proves the superoxygenation of the vinegar; but on the contrary that it is allowable to conclude, that the difference of these two substances is not caused by their state of acidification.

The difference of acetous and acetic acids probably does not depend on the state of their acidification.

X.

*Account of the Imperial Botanic Garden of Schoenbrunn, in the
Vicinity of Vienna.**

IN 1753 the emperor, Francis the first, caused a portion of the garden estab-
ground behind the garden of the castle of Schoenbrunn to be blished in 1753,
prepared for the cultivation of exotics, and of plants remark- by Francis I.
able for their rarity or beauty. By the advice of the cele- Put under the
brated Van Swieten, the famous florist Adrien Steekhoven was care of Adrien
invited to Schoenbrunn from Leyden, who caused many green- Steekhoven,
houses to be constructed there, with a very large and beautiful —Vander Schot
hot-house, and various other buildings. At the same time the first gardener:
Richard Vander Schot, of Delft, was named first gardener, —brings to it
and employed to convey to Vienna a great number of rare and many exotics
exotic plants, brought up in different parts of Holland, from Holland.
and thus at the end of one year the garden was already rich in
valuable plants.

M. Jacquin, who was then at Vienna, went to visit the M. Jacquin sent
garden of Schoenbrunn, to class those plants which had not yet to America to
received a specific denomination; on which occasion he be- collect plants.
came known to the emperor, who proposed to him to travel
at his expence on the continent of South America, and in the
American islands, to enrich the garden with plants from the
most distant countries. Accompanied by the gardener Van
der Schot, he departed from Vienna in 1754; and in passing
through Italy was joined by Jean Buonamici and Ferdinand
Barculi, who were entrusted with the zoological part of the
expedition, by which it was proposed to improve the royal
menagerie, and the cabinet of natural history at the same time.
After having visited the islands of Martinico, of Grenada, St.
Vincent, St. Eustatia, St. Christopher, St. Martin, St. Bar-
tholomew, Aruba, Cuba, Caracca, and Jamaica, he returned
to Vienna in 1759. From August 1757 to the middle of
1759, M. Jacquin could do little for the advancement of Sci-
ence, having been ill of a lientery for four months, of which
he was at last cured at Jamaica. The war which then com-
menced between England and France, also deranged his tra-

Account of his
proceedings in
the West India
islands.

* *Magasin Encyclopedique*, T. 6, p. 552.

vels! The vessel in which he made his voyage was taken, and he was thus obliged to pass a considerable time, against his will, at Montserrat and the desert island of Gonave.

The first cargo of plants shipped, —the second cargo brought by Vander Schot.

In the month of August 1757 the first cargo of plants to the garden of Schöenbrunn was shipped from Martinico, which arrived at Marseilles. In the month of February, 1757, Vander Schot returned also from Martinico, and brought with him from the same island a great quantity of trees and shrubs. All this cargo arrived safe, except some specimens of *Aleliconia*, which were attacked on the voyage by mice. The trees were of the height of a man, and of the thickness of an arm, and sometimes more. The most of them had born fruit in their native soil; their tops had been cut off, and only some of the principal branches were permitted to remain about two feet in length; the shrubs remained in their natural state. To remove those trees from their native earth, a circular trench was dug round each, at a convenient distance, in such manner that there might remain attached to their roots as great a mass of the earth in which they grew as was possible. This mass, which formed a sort of ball, was entirely wrapped up in leaves of the *Mugu*, secured with cords made of the bark of the *hibiscus tiliaceus*, in such a manner that the earth could not fall out.

Method of preparing the trees for carriage, —and of packing them.

Weight of a tree when packed root.

A single tree packed in this manner, weighed commonly an hundred and odd pounds. The balls of earth were moistened a little, with the necessary care, and suspended in the air, where the vegetation soon became apparent.

Method of transporting the packages.

To prevent the earth from being detached from the roots on the way, all the packages were transported in barks to the port of St. Pierre, in Martinico; from hence they were shipped to Marseilles, and from thence brought by sea also to Leghorn, and from this port were carried by mules to Schöenbrunn. This was without exception the richest cargo of living plants which had ever been brought from the hot countries to Europe.

The third cargo of plants shipped, —the fourth, and the fifth.

In the month of August, 1756, Buonamici set off with the third cargo from St. Eustatia to Leghorn. The fourth cargo departed towards the end of the same year. The fifth was shipped from Curacao for Amsterdam, and was accompanied by J. A. Vefuntin, who died in Germany of the dysentery. This cargo was extremely rich in corals and other productions of the sea, which still form some of the most precious ornaments

of the Imperial Cabinet. In the same year, M. Jacquin sent off the sixth cargo, from the same island to Amsterdam. And finally, in January 1759, MM. Jacquin and Barculi departed with the seventh cargo from the Havannah, for Ferrol in Spain, and arrived at Vienna in the month of July. This last cargo was particularly rich in animals of every species.

Thus in the space of a few years the number of plants in the garden of Schönbrunn was considerably increased; for, besides those which had arrived from America, means were found to make many important acquisitions in different other countries. In 1765, after the death of Francis the first, Maria Theresa ordered the garden to be conducted on the same footing that it was before. In 1780, a little while before the death of this princess, it suffered a small but irreparable loss; the gardener, Van der Schot, then very aged, had been confined to his chamber for many weeks by an attack of the gout. Those to whom the management of the plants was entrusted in that period acquitted themselves with great negligence; in one of the coldest nights of that winter, the person who should have taken care of the great hot-house forgot to keep up the fire. In the morning he thought to repair this neglect by heating it to an unusual degree; but the sudden transition from cold to heat killed a great number of fine plants, and among others all the cinnamon trees from Martinico, of which the trunks were as thick as a man's arm, the heads very large, and of the greatest beauty; and also destroyed the plants *Crescentia*, *Achras*, *Annona*, *Portlandia*, and a *Coccoloba Grandifolia*, which was 20 feet high, and whose leaves were of the size of two feet.

This garden also suffered another loss. A considerable collection sent from the isle of France by M. Gere, arrived at Trieste entirely spoiled, the trees all dead, and the seeds im-

prolific.

At this time the emperor, Joseph the second, directed M. Jacquin and M. Born to propose to some men of abilities to undertake a voyage into remote countries. Professor Marter was appointed the conductor of this expedition, and Doctor Stupiez was associated with him, together with the gardeners Boor and Bredemeyer, and the painter Moll. This company of travellers quitted Vienna in the month of April, 1783, and arrived in September at Philadelphia. They travelled over

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The sixth cargo shipped, the seventh brought by M. M. Jacquin and Barculi, 1759.

The garden receives plants from other countries. Maria Theresa patronises the institution.

Many valuable plants destroyed by the negligence of the attendants with regard to the hot house.

A cargo of plants from the isle of France spoiled.

Prof. Marter and others sent to collect in America in 1783.

Several fine plants arrive at Vienna from thence, with M. Bredemeyer.

Pennsylvania, Virginia and Carolina. M. Boor along with M. Schopf, made a journey into Florida, and from thence passed to the island of Providence. M. Bredemeyer returned from Carolina, and passing through England, arrived at Vienna in November, 1784, with several very beautiful plants. Boor, who during his stay at the Bahama islands had collected many rare plants, returned to Vienna in the month of September, 1785. But the painter Moll and Doctor Stupiez were separated from their fellow travellers.

Bredemeyer sent out again, — searches the islands and continent to the river Oronooko.

By the orders of the emperor, M. Bredemeyer, and the gardener Schucht, went towards the end of the year 1784 to rejoin the director of this expedition, M. Marter, who remained all this time in America; they passed over many of the great islands and a part of the continent as far as the mouth of the river Oronoco.

Many rare plants brought back by him and M. Marter in 1788.

In 1789 they returned by way of Amsterdam to Vienna, and brought back many rare and new plants. M. Marter also arrived the same year, by the way of London and Brussels with a new collection of plants.

M. Boor and Scholl sent to the Isle of France.

The emperor had not forgotten the loss of the plants from the isle of France, and commissioned M. Boor and the gardener Scholl to go there, and touch on their way at the Cape of Good Hope. In the month of May 1786, they arrived at the Cape with the Dutch vessels; M. Boor remained there till 1797, and then departed by himself for the isle of France and that of Bourbon. In the month of January, 1788, he returned

M. Boor returns with many fine plants, — leaves some behind at the Cape with Scholl.

to the Cape with 280 cases full of rare plants; and on the 20th of July in the same year arrived at Vienna with a great number of magnificent vegetables; but as all the cases could not be brought in the vessel, the gardener remained at the Cape with the remainder. There has not since been any possibility of getting them to Vienna, as well as many other plants; and the gardener Scholl remains at the Cape from that time, from whence he has sent from time to time seeds and roots. Besides this increase to the garden, the number of

The plants of Schwenk bought.

M. Jacquin, the son, sends many exotics from his travels.

plants was augmented in different manners. Thus, at the sale of the garden of Schwenk at the Hague, the emperor caused all the rare plants to be bought; and likewise M. Jacquin, the son, when he was on his travels over a great part of Europe, sent to Schönbrunn many exotic plants which he found in other gardens.

The emperor Joseph also enlarged the hot-houses, and caused others to be built. In order to bring back Scholl the gardener to Vienna, with the plants which remained in his care at the Cape, the emperor Leopold, in 1791, ordered the gardener Bredemejer and young Van der Schot, (the son of him who had been with M. Jacquin in the East Indies) to sail to the Isle of France, where Cere had collected many plants for the imperial garden, and during their return they were to touch at the Cape to take up all those which remained with Scholl. The captain of the vessel, in which the two gardeners had taken their passage put into Malaga; where they discovered in time that he had bad intentions with respect to them; which obliged them to return to Vienna without performing their commission. After the death of the emperor Leopold, his successor Francis the second had an hot-house constructed, 235 feet long, for the plants of the Cape. A new garden was also established, of which Doctor Host was appointed inspector, and in which were carefully cultivated all the plants which grew in the states belonging to the house of Austria.

Emperor Joseph enlarges the hot houses and builds others.

Emperor Leopold orders Bredemejer to the Cape for Scholl, —he is disappointed of his passage.

Francis II. builds an hot house 255 feet long for Cape plants.

A new garden added for plants of the Austrian states.

By these details may be seen with what care this justly celebrated garden was augmented from the reign of Francis the first, and all astonishment will cease at the riches it contains, and which have furnished materials for different magnificent works on Botany, such as the *Icones plantarum rariorum*, published by M. Jacquin, and above all, that which appeared a few years ago, under the title of *Plantarum rariorum Horti Cæsarei Schönbrunnensis descriptiones et Icones*, in two volumes folio, containing 150 coloured engravings.

Valuable Botanic publications from the garden of Schönbrunn.

XI.

Letter from a Correspondent on the Means of increasing the Action of Sound on the Organs of such as are partially deaf.

To Mr. NICHOLSON.

SIR

ALTHOUGH I am so deaf as not to be able to hear the beating of a watch, unless it be put close to the ear, yet, if I place one end of a stick, or of a metal rod between my teeth

Sounds imperfectly heard rendered audible through a solid applied to the teeth.

and the other end upon the watch, at the distance of several feet, I can hear it very distinctly.

The hearing considerably assisted by pressing the external ear forward;

Or by a trumpet, though not considerably useful.

Probability that an instrument might be invented to act on the teeth or bones of the head so as to magnify sounds.

I know only two methods of alleviating the difficulty of hearing articulated sounds; one is by surrounding the ear, with the hand open, and pressing it forward, the fingers and thumb being fixed behind it; this expedient does more than might be supposed. Another method is, that of the application of a trumpet; which however, is of but little use, constructed as it is at present. The discovery of any instrument to facilitate hearing, by being placed in the mouth (probably after the manner of a tobacco-pipe) would be of great importance to a numerous class of our fellow creatures, whose faculty of hearing is nearly sufficient for common conversation. If an instrument should be invented, which will do any thing at all in this way, our experience in regard to other inventions, encourages an expectation, that improvements will follow: means of assisting human sight have long been devised; little indeed has been done to assist defective hearing; it is however an object deserving of more attention than has been bestowed upon it. If you should be so good as to insert this in your Journal, I indulge a hope, that some of your ingenious correspondents, compassionating the unfortunate situation of those whose hearing is imperfect, may be led to attempt discoveries, the result of which may be of extensive utility. It is desirable to ascertain the best form and size of an ear trumpet, and what metal is to be preferred.

I am, Sir,

Your most obedient Servant,

A. B.

Reference to some disquisitions on sounds in the quarto series of this Journal.

P. S. On referring to my quarto edition of the Journal, vol. IV. page 383, I find something corresponding to my own observation. I shall be extremely obliged if your humanity should determine you to insert the above, as it may be a means of exciting investigation on a subject which is certainly of great consequence.

ANNOTATION—W. N.

Desultory remarks on the modification of sound by means of solid bodies.

A CONSIDERABLE mass of speculation concerning sound, and the means of encreasing its action on the organs of sense is to be found in my annotations on the experiments of Perrole, at p. 416 of vol. I. of the Quarto Series of this Journal. The excellen

excellent papers of Mr. Gough, at page 66 and 160 of vol. X. of the present Octavo Series, concerning the augmentation of sounds, and the speaking trumpet have added considerably to our knowledge of this subject. The memoir of Hassenfratz on the same instrument in our Ninth Volume, p. 283, and another, by the same author, on the Propagation of Sound, at Vol. XI. p. 127, also deserve to be consulted. From the whole consideration of the facts it seems as if the sonorous vibration of the instrument were of much more consequence than has hitherto been suspected; and it seems not improbable, that a large surface exposed to the aerial pulses of sound, and having a tail of communication to be applied to the teeth, or inserted in the ear, might have considerable effect. The use of the external ear, which has excited so much discussion, may, perhaps, be of this kind. The experiment of Dr. Moyes (Philos. Journal, III. 57) of conveying sound to great distance through a string may be added to the other facts; and tends to shew that the sonorous undulation does not require to be transmitted through such bodies as are the most dense, uniform, and elastic. Leather, or felt, or pasteboard, or various other similar materials, are more frequently observed to tremble in the hand at certain particular sounds than many other dense bodies.

XII.

Easy and Correct Method of verifying the Position of a Transit Instrument. By J. S. BUTT, Esq. Communicated by the Author.

To Mr. NICHOLSON.

SIR,

Paragon, December 8, 1805.

A SHORT note having appeared in Mr. Kelly's new edition *Introductory* of Spherics, describing my method of verifying the position of ^{note.} a transit instrument, and thinking an account more in detail may not be unacceptable to your astronomical readers, I am induced to trouble you with it, that you may, if you please, infer it in your valuable Journal.

Your's respectfully,

JAMES STRODE BUTT.

TO

The usual method of adjusting a transit instrument by a circumpolar star.

TO make the line of collimation move in the plane of the meridian, we are desired to observe the transits of circumpolar stars, and if the intervals between the times of their transits are equal, the transit instrument moves in the plane of the meridian: for the axis and line of collimation being previously adjusted, it must pass through the zenith; and if it divides the circle described by any circumpolar star, into two equal parts, it must pass through the pole.

— requires the clock to keep time for at least 24 hours.

But here a difficulty arises which is a probable alteration, or a want of uniformity in the rates of the clock or watch for so long a period as twenty-four hours, or during that portion of time which the observer may require to repeat his observations, so as to be satisfied.

Method independent of this rate and of previous right ascension, &c.

A method independent of the rate of a clock or watch for so long a time, and also entirely of any other previous observations of right ascension, is a desideratum to practical astronomers, and also to those who occasionally amuse themselves by observing time, and the rates of their chronometers, in their present improved state; but who may be unacquainted with astronomical equations, of precession, nutation, &c.

Rule, observe the transits of two different stars, one above and the other below the pole; which differ only a short time, for example, only a few minutes: at any time afterwards repeat the observation upon the stars when their situations as to the pole are reversed. If the difference in time be the same the transit is duly placed, if not it must be altered, &c.

Rule, Observe the difference of transits of any two circumpolar stars, that are situated nearly in the same azimuth, or vertical circle, the one above and the other below the pole: and whose difference of right ascension is nearly 180° ; (namely)

Observe, The transit of α cassiop. above the pole, and immediately after it the transit of ϵ ursæ majoris below the pole, whose difference of transit is not more than 15 minutes, and for so short a time the clock or watch may be safely depended on. Then invert the operation, and observe the transit of α cassiop. below, and ϵ ursæ above the pole. If their difference of transits is the same in both observations, the transit instrument is accurately in the meridian; if not the error may be corrected by altering the position of the instrument till their difference of transit is the same in both observations.

Should the error be great it may be corrected nearly by any of the theorems now in use; (vide Wales on Time-Keepers) or half the difference may be substituted for the error, and by repeated approximation the transit instrument may be accurately adjusted.

The advantage of this method is, that you rely upon the stars keeping $\frac{2}{3}$ of the time, which would otherwise be kept by the clock or watch; and it is of no consequence whether the observations follow one another on the same day or week, provided the instrument is adjusted to the same point of the horizon previous observation, for there is little or no difference in their recession, &c. during an interval of a month.

Advantages.
Most of the time is kept by the stars, and not by the clock.

Another advantage is, that the observations follow each other so soon, that you are not likely to be disappointed by a change of weather; for each pair of observations is complete as far as it goes, which is not the case in the other method, which requiring an interval of twelve hours between each observation, a change of weather is more likely to take place.

The short interval insures against change of weather; &c.

A transit instrument is the basis of astronomy, and whoever has the fixing of it should consider himself independant of every previous observation, and acting entirely upon principle, which is not the case where the adjustment is by previously observed right ascension, and which require reducing to the day of observation; indeed nicely reduced right ascensions are not always in the hands of those who may be wish to be in possession of a simple and accurate method of placing a transit instrument precisely in the meridian.

Other useful remarks.

This method was devised and used by me since 1794, but I have never read or heard of any one using the same.

J. S. B.

N. B. Proper stars in this Lat. are,
 α Cas. and ϵ Ursæ Majoris.
 β Cas. and δ Ursæ,
 γ Cas. and ϵ Ursæ.

Also the stars of Draco and Auriga;
 Cepheus and Ursa,
 Perseus and Draco,

A large comet was discovered at the Royal Observatory Dec. 8, which passed the meridian at 6.^h 24.^m 7. mean time.

Observed right ascension - $353^{\circ} 6' 41''$

Declination south - - - $23^{\circ} 41' 8''$

* * I have since heard that this comet was not again seen, but is supposed to have proceeded southward.—N.

XIII.

A Comparison of some Observations on the Diurnal Variations of the Barometer, made in Peyrouse's Voyage round the World, with those made at Calcutta by Dr. Balfour.*

Barometrical observations between the Tropics by Lamanon and by Dr. Balfour,

THE first of the observations here referred to were made by M. Lamanon, an ingenious naturalist who accompanied Peyrouse, and who has given an account of them, (see fourth volume of the *Voyage*, octavo edit.), in a letter to M. de Condorcet, dated St. Catharine, 5th November 1785. Dr. Balfour's Observations are in the *Asiatic Researches* for 1794, and a short account of them is also inserted in the fourth volume of the *Transactions*, R. S. Edin. Hist. p. 23.

M. Lamanon's observations were made in consequence of instructions from the Academy of Sciences, directing him to keep an exact account of the heights of the barometer in the vicinity of the equator at different hours of the day, with a view to discover, if possible, the quantity of the variation of that instrument, due to the action of the sun and moon, that quantity being there probably as its *maximum*, while the variations arising from other causes are at their *minimum*.

Lamanon used a marine Barometer of Nairne.

M. Lamanon was provided with one of Nairne's marine barometers, which, he says, was so little affected by the motion of the ship, that it might be depended on to the $\frac{1}{10}$ of an inch. In this barometer, he tells us, that from about the 11th degree of north latitude, he began to perceive a certain regular motion, so that the mercury stood highest about the middle of the day, from which time it descended till the evening, and rose again during the night. As they approached the equator, this became more distinctly perceptible; and on the 28th of September, the ship being then $1^{\circ} 17'$ north latitude, a series of observations was begun, and continued for every hour till the 1st of October, at 6 A. M. The following abstract shews the result of the observations on the 28th and 29th.

Regular diurnal change in lower Lats. than 11° . N.

Twenty eighth of September.

From 4 to 10 A. M.	Barometer rose $1\frac{1}{16}$
From 10 A. M. to 4 P. M.	fell $1\frac{2}{16}$
From 4 to 10 P. M.	rose $0\frac{1}{16}$

* From the History of the Royal Society of Edinburgh, 1802.
Twenty.

Twenty ninth of September.

From 10 (28th) to 4 A. M.	fell 1l. $\frac{3}{8}$
From 4 to 10 A. M.	rose 1 $\frac{3}{8}$
From 10 A. M. to 4 P. M.	fell 1 $\frac{3}{8}$
From 4 to 10 P. M.	rose 1

The observation on the 30th were to the same effect; and hence it is concluded that at the equator the flux and reflux of the atmosphere produces in the barometer a variation of about 1 line $\frac{3}{8}$ English, corresponding, as M. Lamanon remarks, to a height in the atmosphere of nearly 100 feet. According to Bernouilli, the action of the sun and moon should produce a tide of seven feet, and according to Mr. de la Place, a tide not nearly so great.

It should be observed, that when these observations were made, the moon was in her last quarter, and the sun a few degrees to the south of the equator. The latitude on the 28th was 50' north, and 11' north on the 29th; in the night between that and the 30th, the ship crossed the line; and on the 30th at noon, the latitude was 42' south: the longitude all this while between 17° 31' and 18° 33' west of Paris, by the time-keeper; so that the coast of Africa, which was the nearest land, was distant about 8° of a great circle, and the American continent about 19°.

The agreement between these, and Dr. Balfour's observations at Calcutta is very remarkable. Dr. Balfour found that during the whole lunation, in which he observed the barometer from half-hour to half-hour, the mercury constantly fell from 10 at night to 6 in the morning; from 6 to 10 in the morning it rose; from 10 in the morning to 6 at night it fell again; and lastly rose from 6 to 10 at night. The *maximum* height is therefore at 10 at night and 10 in the morning, and the *minimum* at 6 at night and 6 in the morning. The only difference is, that in Mr. Lamanon's observations, the *minimum* is stated to have happened about 4 instead of 6. This, however, will not seem a very material difference, when it is remembered, that the instant when any quantity attains either its greatest or its least state is not easily ascertained with precision. From the observations as detailed by M. Lamanon, the time of the *minimum* seems to answer fully as well to 5 as to 4; so that the difference of the results is in

Agreement between these Observations, and those of Dr. Balfour at Calcutta.

every

every view inconsiderable, and their coincidence on the whole, not a little singular. The variations in Dr. Balfour's barometer between the nearest *maximum* and *minimum* is sometimes about $\frac{1}{10}$ of an inch, though in general considerably less.

Whether the cause which produces land and sea winds could produce the regular change.

In the abstract of Dr. Balfour's observations referred to above, it is remarked, that it seems not improbable that these variations of the barometer are connected with the reciprocations of the sea and land winds during the day and night. But whatever may have been formerly the probability of this supposition, it is entirely destroyed by the observations of the

French navigators. These observations were made too far out at sea to leave room for supposing that the land winds had

—neither is it likely that it was caused by tides in the air, as it does not follow the moon.

any influence on the phenomena to which they refer. It is at the same time doubtful, whether those phenomena can be ascribed to the atmospherical tides produced by the sun and moon, as the *ebbing* and *flowing* of the mercury in the barometer appears to have no dependence on the position of those luminaries relatively to one another, but happens, it would seem, constantly at the same hour, in all aspects of the moon and all seasons of the year. The subject is well deserving of a fuller investigation. We should probably before now have had farther information respecting it, if happily the able navigator above-named, and his brave associates, had been destined to revisit their native shores. The cruel fate of an expedition so well planned, and so well appointed for the purposes of science, will never cease to be matter of sincere regret.

Annotation.—W. N.

Probability that the equatorial change is caused by ascending and descending currents in the atmosphere.

I have inserted the foregoing with a view, in some measure, to afford a comparison with Mr. Horsburgh's paper on the same subject, at page 16. It is not without diffidence that I venture to propose a conjecture on this subject, which in fact requires more consideration than I can, at present, bestow on it. Its change seems to me to be governed by the ascent of the air which would take place immediately beneath the sun, if the earth were stationary, and the surrounding descent of the same fluid, of which the circumstances and modifications are so well explained

explained by Prieur in his memoir on the morning and evening dew (at p. 86, vol. IV. of our quarto series.) The considerations there detailed may be easily extended to shew also that the effects must be greatly altered, and, in most instances, obliterated by the vicinity of land; which even changes the regular trade winds into land and sea breezes.

XIV.

Abstract of a Mémoire on the Direction and Velocity of the Motion of the Sun and Solar System. By Dr. HERSCHEL. From the Philosophical Transactions, 1805. (A.)

THE learned author begins his paper by noticing Dr. Maskelyne's table of the proper motions of 36 stars of the first magnitude, and conceives that if this table affords proof of motion in stars in our immediate neighbourhood, the changes of position in minute double stars, many of which are only to be seen by means of the best telescopes, likewise prove that motions are equally carried on in the remotest regions of space.

In 1783, the Doctor deduced from the proper motions of the stars, a motion of the sun and solar system towards λ hercules; and the opinion he then conceived has been much strengthened by the considerations stated in the following pages. Should this doctrine be established, many phenomena may be accounted for, which without it must remain inexplicable.

Though it was proposed, by the admission of a solar motion, to take away many of the proper motions of stars, by investing the sun with a contrary one; our author admits that it will reveal a vastly greater number of concealed real motions than would be necessary to admit, were the sun at rest; and that the necessity for admitting its motion ought therefore to be well established.

The motion of satellites round their primary planets, and of these round the sun, suggests the idea of a revolution of the latter round some other unknown centre; nor are we without hypotheses built upon this conjecture.*

The possibility of a solar motion has been shown by the late

* See *Système du Monde* de Lambert, p. 152, 158. Also Phil. Trans. for the year 1783, p. 283.

Dr. Willson, of Glasgow, upon theoretical principles; and its probability, from reasons of the same nature, by M. de Lalande.

Probability that the sun has progressive as well as rotatory motion,

The rotatory motion of the sun, from which the latter concludes a displacing of the solar centre, indicates a motion of translation in space; for it is not very probable that the mechanical impression which gave the former, should not also occasion the latter. This however can be admitted only as a plausible hypothesis, until we attain a knowledge of the cause of the rotatory motion.

—and the variable stars also.

This argument might be strengthened by closely observing the stars which change their magnitudes periodically; for if these changes arise from a rotatory motion,* a real motion in space may be expected to attend it; and the multitude of these stars is so great, that their concurrent testimony is desirable.

Three sorts of motions of stars.

But setting aside theoretical arguments, the Doctor notices that as all parallaxic motions indicate the observer not to be at rest, it may be necessary to explain three sorts of motions, which will frequently be alluded to in the following discussion.

Parallaxic, real, and apparent.

Suppose the solar system to move towards a certain part of the heavens, the stars, to an inhabitant of the earth, will appear to move in an opposite direction. Let $s p$ (Pl. II. Fig. 1.) represent the parallaxic motion of a star; which, if the star have no real motion, will also be its apparent motion; but if it should have a real motion, which in the same time that it could have gone from s to p , would have carried it from s to r , it will be seen to move along the diagonal $s a$; and $p a$, being parallel and equal to $s r$ will represent its real motion. The triangle $s p a$ is supposed to be formed in the concave of the heavens by three arches of great circles, the observer being in the centre, and $s p$ represents the parallaxic, $p a$ the real, and $s a$ the apparent motion of the star. The situation and length of these arches in seconds of a degree will represent the direction and quantity of each motion; and calling the solar motion S , the distance of the star from the sun d , and the sine of the star's distance from the point towards which the sun is moving ϕ ; the parallaxic motion will be expressed thus: $\frac{\phi \cdot S}{r \cdot d} = s p$.

The largest stars are most fit to shew the sun's motion.

A motion of the sun will occasion parallaxic motions of the stars, and *vice versa*; but to ascertain if parallaxic motions exist, such stars should be examined as are most visibly affected

* See Phil. Trans. for the year 1795, p. 68, and our Journal, XI. 271.

of solar motions; which points out the brightest stars as most proper for the purpose; for any star may have great real motion, but to have great parallaſtic motion it muſt be in the neighbourhood of the ſun.

Parallaſtic may be diſtinguiſhed from real motions by their directions: for, if a ſolar motion exiſt, all parallaſtic ones will tend to a point in oppoſition to the direction of that motion; but real motions will be indifcriminately diſperſed.

Under theſe diſtinctiſons, the proper motions of the ſtars, if the ſun be not at reſt, will be parallaſtic, or compoſed of real and parallaſtic; the latter caſe conſtituting the apparent motion of the ſtar.

Dr. H. next deſcribes the meeting of the arches ariſing from a calculation of the proper motions of the 36 ſtars in Dr. Maſkelyne's catalogue on a celeftial globe, of which ten were made by ſtars of the firſt magnitude, about the conſtellation Hercules; beyond theſe there was no appearance of any other than a promiſcuous ſituation of interſections.—Of the interſecting points, that towards which the ſun moves is denominated the apex of its motion; and as the ſtars will then have a parallaſtic motion towards the oppoſite point, it has received the appellation of a parallaſtic centre.

Interſecting points.	Right Aſcenſion.	Polar diſtance North.	Tabulated reſults.
1. Sirius and Arcturus, in the mouth of the Dragon	255 39 50	36 41 34	
2. Sirius and Capella, near the following hand of Hercules	275 9 32	64 21 48	
3. Sirius and Lyra, between the hand and knee of Hercules	272 23 58	58 23 24	
4. Sirius and Aldebaran, in the following leg of Hercules	263 25 38	44 39 47	
5. Arcturus and Capella, N. of the preceding wing of the Swan	290 0 58	32 7 23	
6. Arcturus and Aldebaran, in the neck of the Dragon	267 2 19	33 57 20	
7. Arcturus and Procyon, in the preceding foot of Hercules	235 3 13	46 21 34	
8. Capella and Procyon, S. of the following hand of Hercules	272 51 49	73 7 56	
9. Lyra and Procyon, preceding the following ſhoulder of Hercules	266 46 49	66 48 11	
10. Aldebaran and Procyon, in the breaſt of Hercules	260 1 29	60 59 34	

Confirmation by
other stars.

As a further confirmation that the parallaſtic motion may be perceived in the motion of the brighteſt ſtars, Dr. H. examined the interſections made by the proper motions of ſome large ſtars of the next order, with the arches in which the ſtars of the firſt magnitude move, and found 15 which gave ſimilar reſults with the former 10, in pointing out the ſame part of the heavens as a parallaſtic centre.

This reſult con-
firmed by double
ſtars.

Changes in the poſition of double ſtars indicate the ſame reſult, and may therefore be more eligibly aſcribed to the effect of parallax, than to admit of ſeparate motions in different ſtars: for, if the alterations of the angle of poſition were owing to a motion of the largeſt ſtar in each ſet, ſuch motions muſt, in contradiction to probability, tend nearly to one particular part of the heavens. This argument derives its validity from the ſame ſource with the former, viz. the parallaſtic motions of at leaſt 23 more ſtars pointing out the ſame apex of a ſolar motion, by their direction to its oppoſite parallaſtic centre.

and by the har-
mony of the
proper motions
deduced from it.

The incongruous mixture of great velocity and extreme ſlowneſs in the proper motions of the ſtars of the ſame magnitude, is removed by the conſideration of parallax from the ſolar motion; and it will be ſeen that there is a general conſiſtency in their motions. The ſame obſervation is alſo applicable with reſpect to the ſidereal occultation of a ſmall ſtar in the Swan.

Investigation of
the direction of
the ſun's proper
motion.

Dr. H. concludes from the foregoing premiſes, that the expediency of admitting a ſolar motion will not be queſtioned, and proceeds to investigate its direction. He begins by proving, that when the proper motions of two ſtars are given, an apex may be found, towards which if the ſun be ſuppoſed to move with a certain velocity, the two given motions may be reſolved into apparent changes ariſing from ſidereal parallax, the ſtars remaining perfectly at reſt. For we muſt not admit more motions than are ſufficient to account for the obſerved changes in the ſituation of the ſtars; and it would be wrong to have recourſe to the motions of two ſtars, when that of the ſun alone may be ſufficient to account for both; which conſideration would be a ſufficient inducement for fixing at once on the calculated apex as well as on the relative diſtances aſſigned to the two ſtars, could other proper motions be, with equal facility, reſolved into ſimilar parallaſtic appearances: but, when a third ſtar does not direct towards the ſame

An apex or pa-
rallaſtic center
is deduced from
the apparent
motions of two
ſtars, ſuppoſed
to have no real
motion.

same apex as the former two, its apparent motion cannot be resolved by the effect of parallax alone; and this difficulty is further enhanced by the number of apices required to solve all proper motions into parallactic ones, increasing, not as the number of stars admitted to have proper motions, but, when their situation is favourable, as the sum of an arithmetical series of numbers, beginning at 0, continued to as many terms as there are stars admitted.

The author here proposes an illustration of his subject by considering the three apices, or intersecting points, No. 1, 2, 5, in the foregoing table.

The distance of Arcturus from the apex of the solar motion is found to be $47^{\circ} 7' 6''$, and its parallactic motion, which is as the sine of that distance $2.08718''$, which is the apparent motion of Arcturus, as established by observation. Namely Arcturus and Sirius.

Admitting Sirius to be a very large star, at the distance of 1.6809 from us, and computing its elongation from the apex of the solar motion at $138^{\circ} 50' 14.5''$, its parallactic motion will be $\frac{\phi \cdot S}{r \cdot d} = s p = 1.11528''$, which also agrees with the apparent motion already ascertained by observation as the proper motion of Sirius.

The distance of Capella from the apex of the solar motion is $30^{\circ} 54' 46''$, and admitting the velocity of the sun towards the before given point, it will occasion a parallactic motion of Capella, in a direction $89^{\circ} 54' 18''$ south-following its parallel, amounting to $2.8125''$. Capella is here taken for a star of the first magnitude, supposing its distance from us to be equal to that of Arcturus. Hence the parallactic motion of another, viz. Capella, is deduced:

By constructing a triangle, the sides of which represent the three motions of every star, not at rest; one of the sides, representing the apparent motion, will be equal to $0.4637''$; the other side, being the parallactic motion, $2.8125''$; and the included angle $18^{\circ} 19' 27''$, from which will be obtained the third side, or the real motion of the star, $2.3757''$. By the given situation of this triangle with respect to the parallel of declination of Capella, the angle of the real motion will be had, which is $86^{\circ} 34' 11''$ north-following the parallel of this star. A composition of the parallactic and real motions in the directions, will produce the annual apparent motion, as established by observation. and by resolving this into the apparent motion and another, this last will be the real (angular) motion, (supposing the other stars to have none.)

It is here observed, that although the proper motion of a
third

But it is not fit that all the real motion should be ascribed to Capella;

but the apex must be taken so as to leave the real motions as small as possible.

Apparent motions of six bright stars

third star is accounted for by retaining the same apex of the solar motion which explained the apparent motions of the other two, yet a great degree of real motion has been assigned to Capella, of which Arcturus and Sirius have been altogether deprived; which shews that the apex of solar motion must be so fixed as to be equally favourable to every star proper for directing our choice. Hence a problem arises, for discovering a point whose situation among three given apices shall be such as if the sun's motion be directed towards it, there may be taken away the greatest possible quantity of proper motion from the three given stars. The intricacy of this problem is, that by a change of the distance of the apex from any one of the stars, its parallactic motion, which is as the sine of that distance, will be affected; so that it is not merely the alteration of the angle of direction which is concerned. From the solution of this problem, a much more complex one would arise, as three stars would certainly not be sufficient to direct the present endeavour to find the best situation of an apex for the solar motion.

It was before shewn that the brightest stars are the most proper for demonstrating the effect of parallax, and that in searching after the direction of the solar motion, the aim should be to reduce the proper motions of the stars to their lowest quantities. The six principal stars, whose intersecting arches have been given, when their proper motions in right ascension and polar distance are brought into one direction, will have the following apparent motions:

tabulated.

Names of the Stars.	Direction of the apparent Motions.			Quantities of the apparent Motions, per Year
Sirius,	68°	49'	40.7" South-preceding.	1.11528"
Arcturus,	55	29	42.0 Ditto.	2.08718
Capella,	71	35	22.4 South-following.	0.46374
Lyra,	56	20	57.3 North-following.	0.32435
Aldebaran,	76	29	37.3 South-following.	0.12341
Procyon,	50	2	24.5 South-preceding.	1.23941
Sum of the apparent Motions,				5.35337

In seeking a solar motion, which requires the least motion in the above six stars, let the line pa , Fig. 1, which represents the real motion, be brought into the situation ma , and the real motion required will then be at a minimum. If by the choice of an apex for the solar motion the angle at s , made by the lines sp and sa , can be lessened, the quantity of real motion required to bring the star from the parallactic line spm to the observed position a , will also be diminished.

Deduction of a solar motion that shall leave the real motions of these the least possible.

It has already been shewn that when two stars only are given, the line sp may be made to coincide with the line sa of both stars, whereby their real motions are reduced to nothing; and that when three stars are concerned some real motion must be admitted in one of them. Now, since all parallactic motions are directed towards the same center, a single line may represent the direction of the effect of the parallax. Therefore, let sP or sS , Fig. 2, stand for the direction of the parallactic motion of the stars; and as in the foregoing table we have the angles of the apparent motion of six stars, with the parallel of each, the direction of the line sP or sS must be computed with the parallels of the same stars, which may be done as soon as an apex for the solar motion is fixed upon. The difference between these angles and the former will give the several parallactic angles Psa or Ssa , required for an investigation of the least quantity, ma , belonging to every star.

A single line may shew the direction of parallactic effect in two stars, &c.

The author exemplifies what he here lays down, by supposing the sun to move towards λ Herculis; and calculating the required angles of the direction in which the effect of parallax will be exerted with the six stars already selected, he obtains the angles of the parallactic motion with the parallel, the difference between which and the former apparent angles with the parallel of each star gives the angles of the apparent with the parallactic motion, as represented in Fig. 2. The lines sa represent the annual quantity of the apparent motions.

Computation, supposing the sun to move towards λ Herculis.

When the situation of the last mentioned angles is regulated as in the figure alluded to, the several lines ma may be drawn perpendicular to SP , and by computation their quantity will be found to be—

Sirius	-	-	0.65427'
Arcturus	-	-	1.28784
Capella	-	-	0.10867
Lyra	-	-	0.11281
Aldebaran	-	-	0.01604
Procyon	-	-	0.04998
Sum			<u>2.22491</u>

The result of this investigation is, that by admitting a motion of the sun towards λ Herculis, the annual proper motions of the six stars alluded to, of which the sum is 5.3537", may be reduced to real motions of no more than 2.2249".

A more favourable apex.

The author here observes, that although the precise place of the best apex is difficult to ascertain, a more favourable one than that above proposed may be obtained: for, by inspection of the figure which represents the quantities of real motion required, when λ Herculis is fixed upon, it will appear that by a regular method of approximation, the line SP may be turned into a situation, wherein all the angles of the apparent motion of the six stars will be much reduced; and it is evident that the parallactic line SP should be turned more towards the line sa, representing the apparent motion of Sirius. He accordingly tries a point near the following knee of Hercules, whose right ascension is 270°. 15'. and north polar distance 54°. 45', see Fig. 3, the quantities required for constructing which are found by the same method as already described in Fig. 2. By a calculation of the angles and the least quantities of real motion, according to this apex, it appeared that the annual motion of the six stars was reduced to 1.4594", which is 0.7655" less than when the apex was λ Herculis.

Its situation.

Supposition that Sirius may be most affected by parallax, as isightest;

or Arcturus, as having the greatest apparent motion.

In the approximation to this point, it appeared, that when the line of the parallactic motion of Sirius was made to coincide with its apparent motion, a certain minimum might be easily obtained of the other parallactic motions. But as Sirius has not the greatest proper motion, the author conceived that another minimum, obtained from the line wherein Arcturus appears to move might be more accurate; as this star from its great proper motion may be more affected by the parallax arising from the motion of the sun. He therefore chose a point not only in the line of the apparent motion of Arcturus, but equally favourable to Sirius and Procyon, the remaining two stars which have the greatest motions.

"If the principle of determining the direction of the solar motion by the stars which have the greatest proper motion, be admitted", observes the author, "the following apex must be extremely near the truth: for, an alteration of a few minutes in right ascension or polar distance either way, will immediately increase the required real motions of our stars. Its place is, right ascension $245^{\circ} 52' 30''$, and north polar distance $40^{\circ} 22''$. The calculation is delineated in Fig. 4. The sum of the least quantities of real motion in this experiment is $0.95595''$, less than the former by $0.50643''$.

Apex on this last
supposition.

In these calculations the author has proceeded upon the principle of obtaining the least possible quantity of real motion to ascertain the most favourable situation for a solar apex, and has proved that the sum of the observed proper motions of the six principal stars may be the result of a composition of two other motions; and that if the real motions were reduced to their smallest possible quantities, they would not exceed 0.9559 .

The Doctor, however, seems to think that these real motions may not be brought down to the low quantities mentioned; and proceeds to shew that this circumstance will not affect the arguments he has used for establishing the method he has adopted; for, although the great proper motions of Arcturus, Procyon, and Sirius, are strong indications of their being affected by parallax, it is not probable that the apparent changes of their situations should be entirely owing to solar motion; but that their own real motions would have a great share in them; and it is evident that in parallaxic motions the distance of a star from the sun is of material consequence; and as this cannot be assumed at pleasure, we are not at liberty to make the parallaxic motion sp , Fig. 1, equal to the line sm ; hence it follows that the real motion of the star cannot be from m to a , but will be from p to a . If, however, ma be a minimum, pa when sp is given, will also be a minimum, and if all the ma 's in Fig. 4 be minima, the sp 's will give the pa 's as small as possible; which is the point desired to be established.

If the nearest
stars be most af-
fected by paral-
lax, their pro-
per motions
may also be
more evident.

In concluding Dr. H. observes, that as it is known that proper motions do exist, and as no solar motion can resolve them entirely into parallaxic ones, we ought to prefer that direction of the motion of the sun which will take away most real motion, and this, as has been shewn, will be done when the right ascension of the Apex is $245^{\circ} 52' 30''$, and its north polar distance $40^{\circ} 22'$.

Conclusion.
Preferable situa-
tion of the apex.

XV.

*New Experiments on the Solution of Sulphur in Alcohol, and the various Kinds of Ether. By M. FAUVRE *.*

Probability that ether would dissolve more sulphur than alcohol.

IN my first note on the solution of sulphur in alcohol, I announced an intention of examining the solvent power of the several ethers upon this combustible; which I had at that time been prevented from, by being obliged to leave Paris for Brussels, to take the office of apothecary to the military hospital. In the paper alluded to, I hinted that it was probable ethers would dissolve sulphur in greater quantity than alcohol; I had been led into this opinion by the results obtained from mixing this mineral with alcohol, at various degrees. I observed, as already stated, that the more alcohol was rectified, the more readily it dissolved sulphur; and *vice versa*, which difference I imagined to proceed from the greater quantum of hydrogen contained in highly rectified alcohol. Knowing ethers to contain less carbon and more hydrogen than alcohol, I had no doubt that they would dissolve a greater quantity of sulphur. The result of the several experiments, which I made under this impression, I am now about to detail: from which it will be perceived that I was not mistaken in my conjecture. I shall also subjoin the new experiments, which I made with alcohol, to ascertain the precise quantity of sulphur it is capable of dissolving, in order to compare the results with those obtained from ethers.

Preparation, &c. of the ethers.

The ethers I employed were prepared with much exactness, and according to the methods recommended by professor Fourcroy. I took care, in each experiment, to ascertain the specific gravity of the ether made use of, the quantity of sulphur dissolved by it, the various results obtained with or without the contact of the sun's rays, and the properties of sulphurated ether.

First and second Experiments.

Sulphuric ether by long digestion without heat took up nearly

In each of two six-ounce matraffes I put two drachms of the flowers of sulphur, prepared in the same manner as for

* Van Mons's Journ. de Chimie, Vol. VI.

the

the experiments mentioned in my first note, viz. nicely washed, and one ounce of rectified sulphuric ether, whole weight was 0.7396. Having secured the mouths of the mat-
 traffes with luting, I put one in a very light place, and the other in a dark place. I shook them every day, and at the end of a month, filtered their contents. On examination the two sulphurated ethers obtained by these operations, presented the following characteristics:

The colour of the ether exposed to the light was scarcely changed; it had a powerful hydro-sulphurous smell, and its taste was disagreeable, and likewise hydro-sulphurous. Mixed with distilled water, it precipitated nothing; but I remarked that the water dissolved a much less quantity of it than when pure. In proportion as the ether became volatilised, the sulphur formed a whitish scum on the surface of the liquid, which at length was precipitated to the bottom of the glass in which the experiment was made. (I shall hereafter mention the quantity of this precipitate.) Put in contact with white metals, it deeply blackened them. (Care must be taken in this latter experiment to close exactly the mouth of the vessel in which the metals are placed in contact with sulphuric ether, on account of the great tendency of ether to be converted into gas by its attraction of caloric from surrounding bodies.) When mixed with a solution of acetate of lead, it gave a pretty considerable black precipitate.

The sulphurated ether prepared without light, possessed all the properties of the other, but in a less degree. It also was less impregnated with sulphur: for, on a repetition of the experiment, and carefully weighing the products, I found that each ounce of the ether prepared in the light contained 35 grains of sulphur; whilst that prepared in the dark held only 29.

Third and fourth Experiments.

Having proceeded as above described, with nitric ether weighing 0.9088, I obtained an ether whose colour was in no degree changed; its smell and taste, though hydro-sulphurous, were not so powerful as those of sulphurated sulphuric ether; mixed with distilled water, it presented the same phenomena, but deposited a less quantity of sulphur. It discoloured white metals less forcibly than the preceding ether;

one-thirtieth part of sulphur in the light; and only one-seventeenth in the dark.

Nitric ether by the same treatment took up nearly one twenty-second part of sulphur in the light; and only twenty-fourth in the dark.

ether; and, in a word, it had all the qualities of sulphurated sulphuric ether, but in a lower degree. It likewise contained a less quantity of sulphur; the result of the experiment made in the light being but 22 grains of precipitated sulphur; and 20 for that conducted in darkness.

Fifth and sixth Experiments.

Muriatic ether took up one thirty-seventh in the light; and only one fifty-third in the dark.

With muriatic ether, weighing 0.7196, proceeding as already described, and at the same proportions, I obtained a sulphurated muriatic ether, possessing all the peculiarities above mentioned, but weaker. It contained only 13 grains of sulphur, when conducted in the light, and $9\frac{1}{2}$ grains when managed in the dark.

Seventh and eighth Experiments.

Acetic ether took up very little sulphur.

Acetic ether weighing 0.8664, dissolved but a very small portion of sulphur, and its qualities were but slightly marked. It contained but three grains of sulphur in an ounce of ether, in the experiment made in the light, and about $1\frac{1}{2}$ grains in that made in the dark.

Ninth Experiment.

Solution of sulphur in alcohol was less charged than that of sulphuric ether.

Having made the foregoing experiments, I wished to ascertain the difference existing between the several ethers and alcohol, in respect to their capacity for dissolving sulphur: I therefore retraced the experiments I had formerly made with alcohol. To avoid the repetition of what has been already communicated in my first essay, I shall here merely state the quantity of sulphur I have been able to dissolve, either by submitting the mixture to a heat less than sufficient to cause the alcohol to boil, or by exposing it to the light, or by placing it in a dark place. For these experiments I used alcohol of 43 degrees.

After digesting for 12 hours over a gentle fire an ounce of alcohol with two drachms of the flowers of sulphur, I obtained 23 grains of precipitate.

Tenth and eleventh Experiments.

On leaving similar mixtures, one exposed to the rays of the sun, and the other in a place impervious to the light, during a month, and proceeding as already described, I obtained

tained 16 grains from the first mixture, and 13 from the second.

After what has here been laid down, it is evident that sulphuric ether dissolved the greatest quantity of sulphur; for, after frequently repeating the experiment, I found the average to be 25 grains in an ounce. Nitric ether and alcohol at 43 degrees, dissolved nearly in the same proportions; and acetic ether the least of any.

It has been long a desideratum in medicine to discover a method of administering sulphur in a state of extreme division, especially in complaints of the lungs and diseases of the skin. With this intent, physicians have recommended it to be dissolved in essential oils, and to form what is known in pharmacy under the title of *balsums of sulphur, terebinthinated, anisated, &c.* These medicaments have the disadvantage of giving to the mixtures into which they enter an almost insupportable taste and smell of sulphurated hydrogen. Sulphurated ether is free from this inconvenience; it may be easily mixed with other potions, to which it gives very little smell; and as the separation of the sulphur is only occasioned by the evaporation of the ether, it may be easily prevented by keeping the mixture to which it is added closely corked. I have already adopted its use with success, administered either upon sugar, or with any appropriate vehicle: several physicians of my acquaintance, for whom I have prepared it, have likewise employed it with advantage: and I hope, ere long, to be able to flatter myself as having added an efficacious medicament to the art of healing.

The sulphurated ether may be also successfully employed to detect the adulteration of wine with preparations of lead: in addition to the facility with which this ether precipitates the lead, in the form of a black sulphur, it possesses the advantage of introducing nothing into the wine that can deceive as to its quality, which sometimes happens even to those who are accustomed to use the solution of sulphur of potash.

I am now occupied in the crystallization of sulphur dissolved in ether, the result of which I shall lose no time in laying before the public.

XVI.

*On the Utility of scientific periodical Publications. In a Letter from Mr. RICHARD WINTER. To which are added, some Experiments of Heat produced by a Blast of Air from Bel-
lows.*

To Mr. NICHOLSON.

DEAR SIR,

Periodical
works are of
modern inven-
tion.

THE advantages derived from scientific periodic publica-
tions, are an acquisition which former philosophers were not
possessed of; and it was not until the last century they were
first instituted. The rapid progress of science and information
since that period, would be a sufficient argument in favour of
their decided utility, without any reference to systematic treat-
ises published, of undoubted merit, and sanctioned by uni-
versal approbation.

Advantages de-
rived from sci-
entific Journals.

To the active and ingenious mind in early life this mode of
information is invaluable. Besides furnishing new ideas to
the young student; they point out the precise state of the dif-
ferent branches of human knowledge; they teach him the
necessary caution for conducting experiments with vigour and
accuracy, instead of drawing conclusions from a few insulated
analyses, or imagining that his data are sufficiently perfect for
establishing new systems. By reading these publications it is
that he will enlarge his general conceptions, and will learn
to emulate the various illustrious characters of all the enlight-
ened countries of the world. In these treatises his views will
not be confined to one object, but he will contemplate a scene
continually varying. The physiology and phenomena of the
animal and vegetable kingdoms; the actions and re-actions of
the different elementary substances in nature, and their com-
binations with each other, will pass in succession under his
observation.

The great physical laws which constitute and maintain the
equilibrium of the world, are inserted in respectable works of
this nature as they are discovered and demonstrated, while
the errors of former philosophers are detected and exposed;
by which means he has an opportunity of ascertaining the va-
lue and correctness of those works he may be already in pos-
session of.

To

To those who consult an Encyclopædia for scientific matter, these publications are of indispensable utility, by continually pointing out the numerous improvements as they become public, and by that means the general system of philosophical knowledge is kept to the level of the existing state of discovery.

To the mechanic a repository of this kind must be highly useful, as the receptacle in which he may record his labours and improvements, and secure to himself the well-earned fame of his discoveries, at the same time that he derives advantage from others following his example in their contributions to the general fund of science.

In short, there is no class of individuals but may profit from this method of extending useful knowledge. The small sum of seven-pence or eight-pence a week to any economical person is trifling, and there is no doubt but every enquirer will find something of which he may abridge himself, in order to become possessed of such an assemblage of facts and opinions. He is as it were making himself intimate with a class of men whose names will be read with admiration by a grateful posterity. It is only by familiarizing the mind with the sublime objects of science, and diffusing them over the face of the earth, that we can expect to establish that spirit of philanthropy and social order, which is so necessary to the happiness of the human race.

I will leave it to your judgment to abridge, or cancel the whole of this paper, as it would perhaps exclude more valuable subjects.

I am, Sir,

With the greatest respect,

Your very humble servant,

RICHARD WINTER.

21, *Bolsover Street,*

Dec. 14, 1805.

The following experiments were made in order to ascertain whether a current of air projected upon a thermometer would increase or diminish the temperature. I made use of a pair of common bellows, the contents of which, when opened, were 95 cubic inches; the diameter of the end of the pipe

The thermometer raised by a blast from bellows.

was

was $\frac{3}{4}$ ths of an inch. The thermometer was adapted to Fahrenheit's scale, and the results of three experiments are exhibited in the following table :

	Number of Blasts.	Time of blowing.	Therm. rose.
Exp. 1.	- 425	6 minutes.	4°
2.	- 222	3 ———	3.75
3.	- 217	3 ———	3.7

The current of air was directed against the bulb of the thermometer. The distance of the pipe out of which the air issued, was half an inch from the bulb. The experiments were repeated with every caution possible for twelve times, and always with the same results.

Mr. Dalton observed (Philos. Journal, III. 160), that the thermometer fell on exhausting the vessel in which it was placed, and rose again on re-admitting the air. It is probable that the rising of the thermometer in my experiments may be referred to the same cause, viz. the greater capacity of a vacuum for caloric than atmospheric air.

XVII.

*An Account of two intersecting Rainbows, seen at Dunglass in East Lothian in July last, was communicated by Professor PLAYFAIR *.*

Large rainbow
where the sun
was 2° high.

And her inter-
secting bow,
over the sea.

“ AT Dunglass, where I happened to be in the beginning of July last, 1799, our attention was called one evening, a little before sunset, to a very large and beautiful rainbow, formed on a cloud which hung over the sea, and from which a shower was falling at a considerable distance to the S. E. The sun was about 2° high, so that the arch was not much less than a semicircle, with its highest point elevated about 40°. At the point where the northern extremity of this arch touched the horizon, another arch seemed also to spring from the sea, diverging from the former at an angle of 3° or 4°, on the side toward the sun.

* Edin. Transf. 1805. History, p. 8.

This arch did not exceed 7° or 8° in length; it was of the same breadth with the principal bow; it had the colours in the same order, and nearly of the same brightness; or if any difference was discernible, it was, that the transition from one colour to another was not made with so much delicacy in the last-mentioned rainbow as in the former.

We recollected that a phenomenon similar to this is described in the *Philosophical Transactions*, as having been seen at Spithead, and that it is ascribed by the gentleman who observed it to the reflection of the sun's rays from the surface of the sea, so as to fall on the cloud where the rainbow was formed. This hypothesis seemed to agree exactly with the phenomenon now before us.

The accidental rainbow, for so it may be called, was seen only at the extremity where the principal arch rose from the sea, and where of consequence, the sun's rays, reflected from the surface of the water, at that moment very smooth, might fall on the drops of rain. The other parts of the cloud could not receive rays so reflected, as the land intervened, and there, accordingly, no vestige of the accidental rainbow was observed.

The accidental rainbow lay, as was already said, on the side toward the sun, and this is agreeable to the hypothesis for the rays that after reflection from the surface of the water fell on the drops of rain, must have come as from a point as much depressed below the horizon, as the sun was at that instant elevated above it. The axis of the accidental rainbow must therefore have made with the axis of the principal, an angle equal to twice the sun's elevation, and its center must have been elevated by that same quantity above the centre of the other, so that if it had been complete, it would have been wholly between the principal rainbow and the sun.

The only circumstance in which the appearances did not perfectly correspond with this hypothesis, was, that the two rainbows did not intersect one another in the horizon, but rather a little above it. This however, ought to have no great weight, as the reflected image of the sun cannot have presented to the cloud a disk so regular and well defined as the sun itself and the

It was a short portion,

and was apparently formed by reflection of the sun's rays

from the smooth water.

Its center was above the horizon;

but the intersection was not quite as low as the horizon.

the accidental rainbow must have somewhat participated of this indistinctness*.

The inclination
of the two arcs
computed,

When phenomena of this kind occur, it would afford a sure means of trying the justness of the explanation, if the inclination of the two bows were observed, and also the sun's altitude at the same time. These two things are necessarily connected; for if we call I the angle of their intersection, E the elevation of the sun, and S the angle subtended at the eye by the semidiameter of the rainbow, if complete, an angle which is constantly the same, and nearly equal to 42° , it is easy to infer from spherical trigonometry, that $\sin \frac{1}{2} I = \frac{\sin E}{\sin S}$.

and was a little
more than the
estimate.

Computing from this formula, the inclination of the two bows in the present instance comes out nearly 5° ; somewhat greater than I was inclined to estimate it by the eye.

Phenomena of this kind can but rarely occur, as the necessary conditions will not often come together. The principal rainbow must be over the sea; the sea itself must extend somewhat on the side toward the sun; it must be smooth and tranquil, and the sun so low that the light reflected from the water may be considerable. Were it ever to happen that the accidental bow was completely formed; the effect could not fail to be very striking.

* As the place of intersection will lie in a plane passing through the eye of the observer and parallel to the plane of reflection; does not this fact afford ground for a suspicion that the reflection, at this low altitude, was made, not from the surface of the sea, but from that of the stratum of vapour which occasions looming, and has been so well treated of by Dr. Wollaston and others, (see our Journal, VI. 46, and elsewhere), and that this stratum was higher farther out at sea than near the coast?—N.

XVIII.

Notice of a Collection of Memoirs which have lately appeared at Paris, being Part of a Work on which the celebrated Lavoisier was employed till the lamented Close of his Life; with a Translation of that Memoir, in which he claims the modern Theory of Chemistry as his own exclusive Discovery. Received from Mr. W. A. Cadell, at Paris.

TO MR. NICHOLSON,

SIR,

Paris, Oct. 27, 1805.

I HAVE translated the two following passages (pages 4 and 5) from a work which has lately appeared in two volumes octavo, entitled *Memoires de Chimie*. They will prove interesting to the readers of your Journal. The first is the notice prefixed to the work by Madame Lavoisier, (now countess of Rumford) it is written with the eloquence of real feeling, and I refer to it for an account of the nature of the work; the second proves completely that the new theory of chemistry is due to M. Lavoisier alone. I also send you the titles of the papers of which the work is composed. I am,

See introductory letter.

Your very humble servant,

W. A. CADELL:

CONTENTS OF THE TWO VOLUMES.

PART. I.—*General Views on Caloric: its Effects; the Manner of measuring it, and the Formation of Liquids and Fluids.*

- 1st mem. on caloric, by *Lavoisier*. Mem. Ac. des Sci. 1777. Contents of the memoirs arranged by *Lavoisier*.
2. On caloric, and the means of measuring its effects, *ib.* 1780.
3. Supplement to the preceding, *Lavois. and Laplace*.
4. On some of the principal phenomena of chemistry.—*Seguin*. Soc. Philom. 1790.
5. On the natural zero. *Seguin*. Annal. de Chim. 1790.
6. On the effects of heat in dilating metals and glass, &c. *Laplace and Lavoisier*.
7. On the passage of solids to a state of liquidity by means of heat. *Lavoisier*.
8. On the action of heat on liquids from their freezing point to that of their vaporization. *Lavoisier*.

On

Contents of the
memoirs arranged
by Lavoisier.

9. On the combination of heat with different evaporable substances and the formation of several fluids. *Lavoisier. Mem. Ac. Sci. 1777.*

10. On the electricity that is absorbed by bodies that pass to the state of vapour. *Lavoisier and Laplace. M. A. S. 1781.*

11. On the action of heat on some aerial fluids from the freezing to the boiling point. *Guyton and Duvernois.*

12. On some substances which are constantly in the state of aerial fluid at the ordinary temperature and pressure of the atmosphere. *Lavoisier.*

13. Memoir on some liquids which can be obtained in an aerial form at a degree of heat a little above the mean temperature of the earth. *M. A. S. 1777.*

14. General views concerning the formation and constitution of the atmosphere.

15. On the cause of some of the principal phenomena of meteorology.

PART II. *On the Decomposition of atmospheric Air, its Analysis and the Conversion of its Principles into the solid or liquid State.*

SECTION I. *On the Decomposition of Air by metallic Substances and the Formation of Oxids.*

1. Memoir on the action of mercury upon atmospheric air. *Lavoisier. In pars. in M. A. S. 1777, p. 186.*

2. On the decomposition of atmospheric air by the oxidation of lead and tin performed by means of a burning glass under a glass receiver. *Lavoisier. Opusc. Chim. chap. 6. pub. in 1773.*

3. On the oxidation of tin in close vessels, &c. *Lavoisier. Read in 1774.*

4. On the decomposition of atmospheric air by iron. *Lavois.*

5. Historical details on the cause of the augmentation of weight that metals acquire when heated with contact of air. *Lavoisier.* It is the paper of which I send you the translation.

SECTION II. *on the Decomposition of Air by simple inflammable Substances which form Acids by their Combustion.*

1. Memoir on the decomposition of air by phosphorus, and the formation of phosphoric acid. *Lavoisier. Opusc. Chim.*

2. Sup-

2. Supplement to the preceding paper. *Lavoisier*, M. A. S. 1777. Contents of the memoirs arranged by *Lavoisier*.

3. Proving that caloric disengaged from vital air during combustion is not possessed of weight susceptible of being estimated.

Process usually employed for obtaining phosphorus, phosphoric acid, and phosphoreous acid. *Seguin*.

5. Memoir on the combustion of phosphorus employed as an eudiometer. *Seguin*.

9. On the decomposition of air by sulphur, the formation of sulphureous and sulphuric acid, and the use of sulphurets in eudiometry. *Lavoisier*.

7. On the process employed in commerce to obtain the sulphureous and sulphuric acids. *Seguin*.

8. On the decomposition of air by charcoal and the formation of carbonic acid. *Lavoisier*. M. A. S. 1781.

2. On the formation of nitric acid by the immediate combination of azotic gas and vital air. *Seguin*.

10. On the eudiometer composed of nitrous gas. *Seguin*.

SECTION III. *On the Decomposition of Air by those simple inflammable Substances which do not form Acids by their Combustion.*

1. Mem. Account of the last experiments on the decomposition and recomposition of water. *Lavoisier* Journal Polytip. February, 1786.

2. Shewing that water is not a simple substance, but a binary combination of hydrogen and oxygen. *Lavoisier*. read in 1783.

3. Shewing by the decomposition of water that it is not a simple substance, and that there are several means of obtaining in abundance, the hydrogen gas, which is one of its elements. *Lavoisier* M. A. S. 1781.

4. Report on the paper of *Seguin*, which treats of the combustion of hydrogen gas with vital air. *Lavoisier*, *Laplace*, &c.

5. On the combustion of hydrogen gas in close vessels. *Fourcroy*, *Vauquelin*, and *Seguin*, read in 1790.

PART IV. *On the principal Phenomena of the Animal Economy.*

1. Mem. Experiments on the respiration of animals, and the change which takes place in air in the lungs. M. A. S. 1777. p. 185.

2. The alterations that the air undergoes during respiration. *Lavoisier*, read in 1785.

3. Memoir, report on a paper of *Seguin's* concerning respiration and animal heat. *Maquer and Fourcroy*.

4. On respiration and animal heat. *Seguin*.

5. On the respiration of animals. *Seguin and Lavoisier*, read in 1790.

Notice prefixed to the Work (by (Mad. Lavoisier) countess of Rumford.

Intention of
Lavoisier to re-
publish his
memoirs,

In the year 1792 M. Lavoisier had formed the design of making a collection of all his memoirs which had been read at the academy during the twenty years preceding. This would have formed in some degree the history of modern chemistry.

In order to render this history more interesting, and more complete he had proposed to insert the memoirs of those, who having adopted his theory, had published experiments in support of it.

in eight
volumes,

This collection was to have been comprised in about eight volumes.

All Europe is acquainted with the cause which prevented their completion.

Parts recovered.

The portion that have been recovered are, the first volume almost entire, the whole of the second, and some sheets of the fourth.

Several men of science expressed a desire for their publication: this was received with hesitation—it is difficult not to be under apprehensions when we are intrusted with the power of publishing the unfinished work of a man justly celebrated. When we have lost the object of our affections and veneration, we should employ an impartial criticism, in order to offer to the public those of his works only which may augment his fame.

Madame Lavoisier
has printed them.

We should have persisted, and these fragments would not have appeared, had they not contained a memoir of M. Lavoisier (inserted below page 5) in which he reclaims the modern theory of chemistry as belonging to himself, and states the facts in support of his claim.

It is consequently a duty towards him to fix the opinion of men of science concerning this point.

Lavoisier was em-
ployed on this

Their indulgence is requested for the errors that may exist in some other parts of the collection. It will be granted when

they are informed that the greatest part of the proof sheets were revised in the last days of the author's life; and that whilst he knew that his assassins were premeditating his death, M. Lavoisier, calm and intrepid, employed his last moments in a work which he considered as useful to science, and gave a great example of that serenity which a virtuous and enlightened man can preserve in the midst of the most severe calamities.

PART II.—SECT. I. *Fifth Memoir*, (Tom. II. p. 78.)

Historical Details concerning the Augmentation of Weight which the Metals acquire when heated with Contact of Air. (By Lavoisier.)

IT is not my object in this paper to give a complete history of the different opinions that have been successively adopted by the chemists and natural philosophers on the cause of the augmentation of weight in metals exposed to the action of heat; such a history would only serve to shew how much the minds of men are susceptible of being led astray when they give themselves up to the spirit of theory, and how easily we are deceived by reasoning, if it is not perpetually rectified by experiment. John Rey, a physician (*medecin*) little known is one of the first authors who has written on this subject; he lived in the beginning of the 17th century at Bugue in Perigord, and kept a correspondence with the small number of persons who cultivated the sciences at that time.

Limit of this historical memoir.

John Rey an early writer on combustion.

Neither Descartes nor Pascal had yet appeared; the vacuum of Boyle, and that of Toricelli, the cause of the ascent of liquids in tubes void of air, were unknown; experimental philosophy did not exist; a profound darkness reigned in chemistry. Nevertheless, J. Rey, in a work published in 1630, with a view of determining the cause of the augmentation of weight which takes place in lead and tin during their oxidation, displayed views so profound, so analogous to the facts which have been since confirmed by experiment, so conformable to the doctrines of saturation and affinity, that for a long time I could not help suspecting that the essays of J. Rey had been composed at a much later period than that announced on the title page of the book.

His philosophy far exceeded that of his contemporaries.

J. Rey, after having refuted successfully, not by facts (for at that time the art of making experiments was in its infancy) but by conclusive reasoning, the different causes to which the

He contends that metals gain weight from the air in oxidation.

augmentation of weight of metallic oxides might be attributed, expresses himself as follows in his 13th essay: "to this question then, supported on the grounds already mentioned, I answer and maintain with confidence, that the increase of weight arises from the air of the vessel, which is condensed, rendered heavy, and adhesive, by the violent and long continued heat of the furnace; this air mixes itself with the calx (frequent agitation conducing) and attaches itself to the minutest molecules, in the same manner as water renders heavy sand which is agitated with it, and moistens and adheres to the smallest grains.

He opposes other current opinions. J. Rey combats in this work the opinion of Cardan (*lib. 5. de subtilitate*) on the augmentation of weight of metallic oxides; that of Scaliger, that of Cœsalpinus, who ascribed this augmentation to a foot condensed and reflected by the furnace, which foot, according to their opinion, fell down upon the metal. He shews likewise that the augmentation of weight proceeds neither from the vessel, nor from any emanation of the charcoal, nor from the humidity of the air. It is difficult to conceive how J. Rey could attain to these conclusions by the force of reasoning alone, without experiment, and ignorant as he was of many of the preliminary date.

His doctrines were not received by Boyle, Boyle and some of his cotemporaries created the new science of natural philosophy, of which the ancients had not the slightest notion, the work of J. Rey was entirely forgotten.—Boyle, in his treatise on the weight of flame and of fire, published in 1670, that is 10 years after the publication of Rey's work, makes no mention of it; proceeding upon some illusory experiments, he still maintained at that time that the augmentation of weight which the metals acquire by their oxidation arises from the fixation of fire.

—nor by Lemery.

Lemery, who was an exact and scrupulous observer, embraced the same opinion: he attributes the oxidation of metals and their augmentation in weight which accompanies that operation, to the combination of igneous particles with the metal.

Opinion of Charras,

Charras, cotemporary of Lemery, ascribed that augmentation to the acids of the wood and charcoal, which as he supposed penetrated the vessels and entered into combination with the metals. Since that time the same acid of wood and char-

coal has re-appeared under the name of *acidum pingue*, igneous acid, and under several other denominations which it would be superfluous to enumerate.

Stahl could not be ignorant of the fact that metals exposed to heat acquire an increase of weight; and yet he not only did not attempt to explain it, but also the system under which he classed the whole of the chemical phenomena, and which after him has been so much extended, is absolutely in contradiction with this capital fact —and of Stahl.

Stahl supposed that the metals are composed of a metallic earth, and an inflammable principle, which he named phlogiston; he pretended that they lost this principle by their oxidation, and that they could not return to the metallic state unless the phlogiston they had lost was restored to them.

It was difficult to imagine how the metals acquired weight, whilst, according to Stahl's doctrine, they lost a part of their substance, and on the other hand, how they diminished in weight, when they recovered one of the principles which they had lost; it was one of the chief difficulties that could be proposed against the theory of Stahl, this difficulty however, has not hindered the theory from having a success of limited duration. Difficulties of the phlogistic system.

Gayton Morveau has made some unsuccessful efforts to palliate this contradiction, in his dissertation on this subject, under the title of *Degressions Academiques*; he supposes that phlogiston is lighter than atmospheric air; and he concludes that all bodies that acquire phlogiston should lose a part of their weight; that, on the contrary, those which lose phlogiston should augment in weight. This explanation would have been tenable, had the augmentation of weight acquired by the metallic oxides been equal only to the weight of the air displaced; or, which is the same thing, if it had disappeared on weighing in vacuo; but the augmentation is much too considerable to admit of being attributed to that cause, since in some metals it exceeds one third of their weight. It is necessary then, either to give up the explanation of Gayton Morveau, or to suppose that phlogiston has a negative gravity, a tendency to recede from the centre of the earth, a supposition incompatible with all the facts admitted by the disciples of Stahl. Morveau's endeavours to remove them.

Such was the state of the science when a set of experiments undertaken in 1772, upon the different kinds of air or gas History of the authors experiments.

which are disengaged in effervescence, and a great number of other chemical operations, discovered to me demonstratively the cause of the augmentation of weight that the metals acquire when exposed to heat. At that time I was not acquainted with J. Rey's work upon the subject, published in 1630; and had I known it, I should have considered his opinion in the light of a vague conjecture, which did honour to the genius of the author, but required the attention of chemists in order to ascertain the truth of the opinion by experiment. I was young, I had newly entered the lists of science, I was desirous of fame, and I thought it necessary to take some steps to secure to myself the property of my discovery. At that time there existed an habitual correspondence between the men of science of France and those of England; there was a kind of rivalry between the two nations, which gave importance to new experiments, and which sometimes was the cause that the writers of the one or the other of the nations disputed the discovery with the real author; consequently I thought it proper to deposit on the 1st of November 1772, the following note in the hands of the secretary of the Academy. This note was opened at the meeting of the 5th of May following, and mention of these circumstances marked at the top of the note. It was in the following terms:

He finds that sulphur and phosphorus gain weight by combustion, and that the gain is from air absorbed. He infers that the phenomenon is general and disengages air from litharge on reducing it in closed vessels.

"About eight days ago I discovered that sulphur in burning, far from losing, augments in weight; that is to say, that from one pound of sulphur much more than one pound of vitriolic acid is obtained, without reckoning the humidity of the air; phosphorus presents the same phenomenon; this augmentation of weight arises from a great quantity of air, which becomes fixed during the combustion, and which combines with the vapours."

"This discovery, which I confirmed by experiments which I regard as decisive, led me to think that what is observed in the combustion of sulphur and phosphorus might likewise take place with respect to all the bodies which augment in weight by combustion and by calcination, and I was persuaded that the augmentation of weight in the calces of metals proceeded from the same cause. The experiment fully confirmed my conjectures; I operated the reduction of litharge in close vessels with Hales's apparatus, and I observed that at the moment of the passage of the calx into the metallic state, there was a disengage-

disengagement of air in considerable quantity, and that this air formed a volume at least 1000 times greater than that of the litharge employed. As this discovery appears to me one of the most interesting which has been made since Staahl, I thought it expedient to secure to myself the property, by depositing the present note in the hands of the secretary of the academy, to remain secret till the period when I shall publish my experiments.

© (Signed) LAVOISIER.

Paris, 1st November, 1772.

Comparing this first note with that which I had deposited at the academy the 20th of October preceding on the combustion of phosphorus, with the paper which I read at the academy of the public meeting of Easter, 1773, and lastly with those that I have published successively, it is easy to perceive that I had conceived so early as the year 1772 the general idea of the theory of combustion which I have since published.

This theory which I have considerably developed in 1777, —which was and which almost at that period I brought to the degree of perfection in which it is at present, was not begun to be taught by Fourcroy till the winter 1786-1787; it was not adopted by Guyton Morveau till a later period; and Berthollet wrote still in the language of the phlogistic doctrine in 1785. This theory then is not, as I hear it called, the theory of the French chemists; it is *mine*, and it is a property which I reclaim before the tribunal of my contemporaries and of posterity. Others undoubtedly have contributed to its perfection, but I hope that no one will dispute with me, all the theory of oxidation and combustion; the analysis and decomposition of ~~air~~ by metals and inflammable bodies; the theory of acidification; more accurate knowledge on the nature of a great many acids, and particularly the vegetable acids; the first notions on the composition of vegetable and animal substances; the theory of respiration, in which Seguin co-operated with me; the present collection will present all the papers on which I found my claims; the reader will judge.

Whence he vindicates his right to the modern theory of combustion in 1772.

not adopted by other chemists till many years afterwards.

The claim specifically stated.

ANNOTATION.—W. N.

It was my intention to have pointed out how far the earlier chemists, as well as some of the contemporaries of this deservedly early inventors

of the theory of edly celebrated philosopher, are intitled to rights which will greatly modify the unqualified claim he has made. I cannot now say, whether Rey did, or did not *make experiments*, but whether he did or not, he certainly must have founded his introductions upon facts; and between the observation of well established facts, and the making of direct experiments there seems to be no essential difference. How it has happened, that the great Robert Hooke, who had investigated the modern theory of combustion in 1664 and published it in an ample detail on his *micrographia* in 1675*, and John Mayow, who soon afterwards, or about the same time established the same doctrine, and extended it to physiological results, are overlooked by our author, appears to require some discussion. I shall take an early opportunity of resuming this subject.

XIX.

On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, Esq. F. R. S. Professor of Chemistry in the Royal Institution†.

Acid of borax
very useful in
analysis.

I HAVE found the boracic acid a very useful substance for bringing the constituent parts of stones containing a fixed alkali into solution.

It combines
with earths by
ignition and
quits them to
mineral acids.

Its attraction for the different simple earths is considerable at the heat of ignition, but the compounds that it forms with them are easily decomposed by the mineral acids dissolved in water, and it is on this circumstance that the method of analysis is founded.

Processes.

The processes are very simple.

Pulverize the
stone and fuse
with two parts
boracic acid.

100 grains of the stone to be examined in very fine powder, must be fused for about half an hour, at a strong red heat, in a crucible of platina or silver, with 200 grains of boracic acid.

Digest with
weak nitric acid.

An ounce and half of nitric acid, diluted with seven or eight times its quantity of water, must be digested upon the fused mass till the whole is decomposed.

Evaporate.

The fluid must be evaporated till its quantity is reduced to an ounce and half or two ounces.

* Copied in our Journal quarto series III. 479.

† Phil. Trans. Part II. for 1805.

If the stone contain silica, this earth will be separated in the process of solution and evaporation; and it must be collected upon a filter, and washed with distilled water till the boracic acid and all the saline matter is separated from it. Silica if present will separate.

The fluid, mixed with the water that has passed through the filter, must be evaporated, till it is reduced to a convenient quantity, such as that of half a pint; when it must be saturated with carbonate of ammonia, and boiled with an excess of this salt, till all the materials that it contains, capable of being precipitated, have fallen to the bottom of the vessel. Precipitate the rest with carbonate of ammonia.

The solution must then be separated by the filter, and the earths and metallic oxides retained.

It must be mixed with nitric acid till it tastes strongly sour, and evaporated till the boracic acid appears free. Add nitric acid to the clear liquid.

The fluid must be passed through the filter, and subjected to evaporation till it becomes dry; when, by exposure to a heat equal to 450° Fahrenheit, the nitrate of ammonia will be decomposed, and the nitrate of potash or soda will remain in the vessel. Separate the boracic acid by evaporation. Decompose the nitrate of ammonia by heat.

It will be unnecessary for me to describe minutely the method of obtaining the remaining earths and metallic oxides free from each other, as I have used the common processes. I have separated the alumina by solution of potash, the lime by sulphuric acid, the oxide of iron by succinate of ammonia, the manganese by hydrosulphuret of potash, and the magnesia by pure soda.

XX.

Some Facts and Speculations on the luminous Phenomena of Electricity. W. N.

ABOUT eighteen years ago, I was considerably occupied in experiments upon electricity, many of which were communicated in 1789, to the Royal Society, and were published in the transactions. In the twenty-third section of that communication, some account is given of certain changes which take place in the luminous appearance of metallic balls when electrified; but the phenomena were not delineated, because I reserved them for another opportunity. After so long an interval

Communication
to the Royal
Society on
electricity.

terval of time, I now present them to the reader from my notes, and the sketch then made.

Three appearances of an electrified ball. It gives flashes; is then luminous, and then gives flashes of another kind.

Sept. 19, 1787. A small ball in the state of electricity called positive, threw out flashes or ramified sparks; and when the intensity was increased, the ball itself became luminous, at the same time emitting the flashes. When the electricity was still more strongly excited the flashes ceased, and a circle of light, extending about 15 degrees round the point farthest from the stem, was seen on the ball, and a strong wind proceeded from it.

Experiment with a ball $1\frac{1}{2}$ inch diameter.

A ball of one inch and a half diameter was used; and electricity communicated by means of a cylinder nine inches diameter, having its cushion eight inches long. The excitation was strong enough, by slow turning with a single winch, to throw out large brushes of light. When the rotation was quicker, the flashes disappeared, and the circle of light was seen, having a bright speck moving irregularly round in its periphery. Quicker turning threw out brushes of light very different from the others: These were less luminous in the branches; many started out at once with a hoarse sound. They were greenish at the point or surface of the ball, reddish in the stem, and ramified sooner. Half a dozen were sometimes seen flashing out at once.

Description.

Experiment with a much smaller ball. It became luminous, and acted like a point.

A ball of four tenths of an inch in diameter was used. Moderate excitation produced a dense brush of light about two inches in length. With stronger electricity the brush disappeared, and the upper half of the ball became luminous. When the excitation was still stronger, more than half of the ball was luminous, as represented *Fig. 3, Plate I.* and sometimes a ramified flash struck out from the top. Other flashes were sometimes seen sideways when the electricity was strongest of all; but this happened seldom.

The light was faint, and seemed to be about twice the diameter of the ball. It extended more than half way down, and spread most sideways.

A large ball $2\frac{1}{2}$ inch. diam.

When a larger ball of two and a half inch diameter was used, the brushes of light flew out from three or four stems together to the length of about six or seven inches, making a hoarse noise; but they could not be made to disappear, though they seemed now and then to cease for a moment when the turning was most vigorous.

The next day, when the excitation was very nearly, but not quite, as strong, it was observed that the order of these appearances could be effected by the assistance of a metallic point. Plentiful brushes were thrown out from a three inch ball, but they could not be made to disappear. When a pointed wire or a small metallic ball was presented, the effects were as follow :

The point being at a great distance, the root of the brush had a luminous circle of lambent light round it on the surface of the ball. When the point was nearer, the brush disappeared, and nothing was seen but an exceedingly bright speck on the surface of the ball, which was sometimes stationary and sometimes moved about. When the point was still nearer, the speck threw out ramified sparks of the second kind, at the same time that a lambent luminous circle appeared. The speck was never in the center of the circle, but moved at a distance round the circle, irregularly, sometimes the one way and sometimes the contrary, and was sometimes stationary.

These two orders of brushes were entirely the same as those of the day before. The luminous brush which first appeared had a straight stem, then a broken or less luminous part, beyond which loose cotton-looking fibres flew off in radial directions, as at *Fig. 1, Pl. I.* The latter ramified sparks had a straight central stem, out of which well defined branches issued nearly at right angles. They much more closely resembled a tree bare of leaves.

The second brush was not larger, but rather less in its dimensions than the first.

When the ball of four-tenths of an inch was held at a certain distance from the two and a half inch ball, when electrified, the first kind of brush was seen on the side farthest from the small ball, at the same time that the second kind of spark or brush flew out towards the small ball, and the lambent luminous appearance was seen on the surface.

These are the general facts; but I have no doubt but they would present many modifications upon being repeated.

These facts may serve to assist our meditations with regard to the nature of the electric spark. In a late paper by Mr. Biot, given at page 214 of our Vol. XII. the author makes an ingenious conjecture, that the light and heat in this phenomenon may have been produced by mechanical compression

Modification of the phenomena,

by the vicinity of a point or small ball.

More particular description of the luminous appearances.

All the phenomena visible at once.

Remarks on the electric spark.

Whether Biot's theory of luminous condensed air can be supported.

Wartire's fire-ball.

Combustion of flint and steel,

requires a very minute portion of metal:

Electric temperature is extremely elevated;

All metals lose by electricity, and the spark passes only between combustible bodies.

Hence probably it was part of the body set on fire.

Fire-Balls, &c. may be electric sparks;

and the spark a fire-ball.

Facts are more wanted than conjectures.

of the atmospheric air. Whether this supposition can be reconciled to the appearance of the spark in oil, and to some atmospheric phenomena, in which we are told of luminous balls moving apparently with little velocity through the air, and particularly that slowly-moving artificial fire-ball, produced once, and only once, by Wartire, as narrated in Priestley's work on air, may admit of question. When we consider that a particle of iron, cut off and set on fire in the common action of striking a light, appears, from the vivacity of its combustion, to be a body of considerable magnitude, though the usual quantity of metal would not form a ball of one thousandth of an inch in diameter; when we consider the prodigious elevation of temperature indicated by the explosion of wires of all metals by the electric shock, particularly in those beautiful and striking experiments which Van Marum has published; and lastly, when we call to mind that a metallic chain loses part of its weight every time a shock is passed through it, and that the spark is never seen to pass between incombustible bodies—considerable reasons will present themselves in favour of a modified supposition, that the electric spark may consist of, or be accompanied by, a portion of the body from which it proceeds.

Are not the atmospheric fire-balls or luminous meteors, the shooting stars and the stones which have fallen from the atmosphere, electric sparks upon a scale of immense magnitude?

If any luminous ball were to pass with a swift angular motion over the field of view, it would have the appearance of a line or streak of light. If it were to break in pieces many divergent streaks would be seen. May not the electric brush be a phenomenon of this description on a small scale?

It would not be difficult to apply this speculation to the figures 1 and 2 before us; but as we are more in want of facts than of conjectures, and as it may be hoped that some of my readers who have the means and the time will pursue this investigation, I shall for the present conclude.

SCIENTIFIC NEWS.

Anatomical Cabinet.

THERE has appeared at Berlin, a complete description of Anatomical cabinet, the anatomical cabinet of M. Walter, which the king has purchased, almost a year ago, for the sum of 400,000 francs. This catalogue is composed of sixty-two printed sheets,

Shower of Peas,

Dr. Hiem, of Berlin, has published a note, in which he explains that the peas, which were said to have fallen from the atmosphere in a shower at Landschut, in Silesia, were merely tubercles which are separated from the roots of several plants. Those in question, according to the Doctor, were afforded by the roots of the aquatic plant *Ranunculus Ficaria*. He pretends that an enormous mass of these tubercles may have been formed in certain cavities, whence they might be carried to a distance by the whirl or eddy of strong wind. He supports his opinion by the accounts of showers of this nature given by the celebrated Klaproth in his Journal of Chemistry. The Doctor concludes by remarking, that these tubercles contain a farinaceous substance equal in goodness to that of potatoes, and recommends an attention to the ficaria for this purpose,

Universal Language,

THE Celtic academy, in a sitting of last April, made proof of a new discovery by one of its members; which gives the power of corresponding, and discoursing, with men, whose language is unknown, with expedition, without previous study, any expence, the least trouble, or the smallest labour of the mind. The proof made at that sitting by twenty-five academicians, on the languages of Europe, ascertained, that by the aid of this invention, a man may travel any where without an interpreter, demand what he wants, discourse on whatever subjects can interest any sort of travellers, and even express metaphysical thoughts. It is intended to make this discovery public at the return of the Emperor.

The above account has appeared in several publications of credit, but it is probable the account is exaggerated in several respects.

Turkish Edict in Favour of Science.

Turkish edict in
favour of sci-
ence.

THE Grand Seignor has constituted Prince Moroufi; by a diploma written with his own hand, director general of the hospitals of his empire, and inspector of the schools of medicine, mathematics, and *belles lettres*, which his highness is engaged in founding with all possible dispatch. This diploma is remarkable for the great praises of the sciences made in it by the Grand Seignor; as they hitherto have been in no great favour with the Mahometans. In rendering justice to the skill of the Christian physicians, who have studied at the universities of Halle, of Padua, and of Montpelier, the Grand Seignour remarks with much truth, that these physicians, when brought into foreign countries, often commit great errors on account of the difference of the temperature of the climates; from whence he concludes that, in order to practise medicine well, it is necessary to study in the country where the profession is to be exercised.

Coptic Manuscripts.

Coptic manu-
script.

THE celebrated Danish antiquarian, M. Zoega, is daily occupied at Rome in completing his catalogue of Coptic manuscripts in the Borgheze museum. He intends afterwards to publish a new topography of ancient Rome. It is probable this work will be printed in Germany, because it will require numerous engravings, which no Italian bookseller would choose to go to the expence of. It is, however, not believed that M. Zoega will occupy the professor's chair, which has been granted him at the University of Kiel, as he is too much accustomed to the fine climate of Italy to leave it willingly.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1806.

ARTICLE I.

*On the Cause of Fairy Rings. In a Letter from
Mr. FLORIAN-JOLLY.*

To Mr. NICHOLSON.

*Assembly-house, Laytonstone, Essex,
January 13.*

SIR,

SEEING by the letter of Mr. Gough inserted in the last Number of your Journal, that the cause of fairy-rings is not yet agreed upon among naturalists, I beg leave to submit to their consideration a few facts which I had occasion to remark some years ago, during a summer residence in Hampshire.

The park of Broadlands, Lord Palmerston's seat, near Rumsey, was divided into three principal inclosures, formed by hurdles only. One of these had been lately mowed; there were cattle grazing in the next; and the other, which had afforded winter and spring fodder to some horses kept at grass, was then left to grow for an autumn crop. This last exhibited an immense number of fairy rings, some perfectly circular, some forming irregular curves, and others nothing but small
The phenomenon of fairy-rings in one division of Broadlands park and none in other parts.

I round

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round patches: In all of these the grass grew more luxuriant and of a deeper hue: No other fungus was to be found in any of them but the esculent mushroom. In the part lately mowed, and in that where the cattle were grazing, there was not the least appearance of fairy-rings.

Another field
abounding with
them.

In the course of subsequent perambulations, I observed in a grass field situated on the top of the first high ground upon the road from Rumsey to Salisbury, appearances nearly similar to those exhibited in the growing grass of the park. There had been all summer, and there were still horses grazing in this field: The fairy-rings were numerous, but the grass in the rings and patches, instead of being more luxuriant, was completely dry and blasted, and there grew two or three different fungi, all of them of those sorts which are reckoned noxious.

Fungi were not
produced by
electricity,

That the fairy-rings at Broadlands were not the effect of electricity, appears to me beyond all doubt, since one part only of the park exhibited them, while the rest of the contiguous grounds, divided from that part by nothing more than a row of hurdles, did not shew any such appearance: otherwise it must be contended, that the electrical phenomena might take place on one side of the hurdles and never on the other, a predilection truly singular, and, I should think, difficult to be accounted for.

caused by the ex-
crement of the
horses.

Another fact which I have repeatedly observed since that time, has led me to suspect that the fairy-rings, their different appearances, and the various species of fungi found in them, might be produced by no more uncommon cause than the excrements of the horses.

Argument from
the appearances
in hot-beds.

The hot-beds made of horse-dung, which I have had several times in my garden, have generally produced in succession the same fungi which are to be found in the different states of the fairy-rings. Whilst the beds are yet new, the fungi are of the same noxious species as I saw in the dry blasted fairy-rings, but when they grow cooler and more matured, esculent mushrooms begin to grow naturally, and although no spawn was ever put in the bed.

I have also remarked, that horse-dung produced in some seasons an immense quantity of mushrooms, and hardly any in others: This might perhaps be attributed to the different quality of the hay on which the horses had fed; and this might explain why fairy-rings are to be found in some pastures rather than in others.

That

That fairy-rings should be produced by the excrements of Experiment horses, may be illustrated by a very simple fact, which it is in with oil, to illustrate the deduction. the power of every person to observe. If you let fall some oil upon a marble slab, or some other liquid upon some substance that will imbibe it, you will see it gradually spread round in a more or less regular form; sometimes assuming the appearance of a patch, and frequently continuing to flow from the center to the circumference, where it accumulates in a much greater proportion than in the inner part of the circle, taking thus the form of a ring.

This accumulation of the fluid at the circumference may be easily explained. As the fluid expands, the pressure from the center becomes gradually less, till at last there is no sufficient force to overcome the resistance opposed by the dry parts of the solid substance which has imbibed it: yet, in consequence of the first impulse, the fluid will continue to flow from the center through the small channels already opened, and will thus accumulate in greater quantities at the boundaries where its expansive motion is stopped.

The excrements of horses, diluted by the rains and imbibed Application, in the soil, must have an effect similar to that just described. This effect must, besides, greatly depend upon the nature of the soil and the facility with which it is pervaded by the fluid; hence the constant appearance of fairy-rings in some pasture-grounds, while none are ever to be found in others.

Should you, Sir, consider these remarks, and the deductions which they have suggested to me, as likely to throw some light upon the cause of fairy-rings, you are welcome to make any use of them you may think proper.

I am, Sir,

Your obedient humble servant,

J. FLORIAN-JOLLY.

Experiments on the Magnetism of slender Iron Wires.

By JOHN GOUCH, Esq.

To Mr. NICHOLSON.

SIR,

Middlesex, January 9, 1806.

A general maxim
in magnetism
stated.

THE general phenomena of magnetism have given rise to a maxim which shall be here stated in the words of a judicious writer on the subject. "The magnetism acquired by being placed within the influence or the sphere of activity of a magnet in soft iron, lasts only while the iron continues in that situation; and when removed from the vicinity of the magnet, its magnetism vanishes immediately; but with hard iron, and especially with steel, the case is quite different; for the harder the iron or steel is, the more permanent is the magnetism, which it acquires from the influence of a magnet." Cavallo on Magnetism, London, 1787, p. 30.

Remarks on this
maxim.

This proposition is of great utility in the science, for it explains a variety of relations betwixt the magnet and ferruginous bodies, but I have observed one phenomenon that appears inexplicable on the principle, and consequently may be said to offer one exception to the general proposition. As my experiments on the subject are very easy, it seems adviseable to deliver the leading circumstances in the form of so many precepts because this method will assist any one desirous of pursuing the enquiry, to repeat them with ease.

An experiment
consistent with
the maxim.

Experiment 1. Apply either pole of a strong magnet to one end of a short horizontal bar of clean soft iron, and a particle of iron equally soft to the other end. This particle will remain suspended at the extremity of the bar until the magnet is withdrawn; but the removal of this power will dissolve the connection subsisting betwixt the two pieces of iron, and the particle will drop off immediately.

An experiment
contradicting
the maxim.

Exp. 2. The preceding experiment confirms the maxim stated above, when conducted according to the foregoing directions; but let it be repeated with the following alteration, and it will contradict the general proposition. In place of the particle of soft iron, substitute a piece of iron wire of number 39 in the wire drawers scale, the weight of which may amount to two or three grains. The removal of the magnet will

Fig. 1
Diagram illustrating the method of determining the position of the stars in the constellation Lyra.

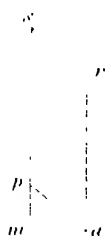
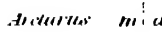
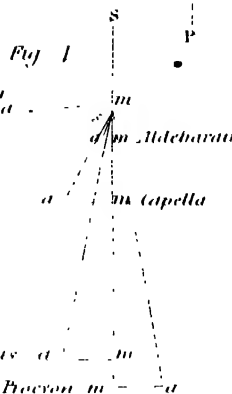
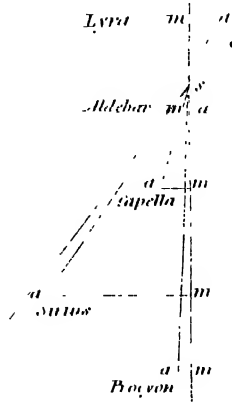
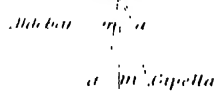
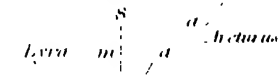


Fig. 3



will not break the connection formed by its presence between the bar of soft iron and the wire; for the latter will remain attached to the end of the former, by the extremity which was first brought into contact with the iron; if the piece of wire be removed from the end of the bar, the magnetic connection may be revived by replacing it immediately. The same thing will happen if the wire be expeditiously transferred from the first bar to another rod of soft iron; but it loses its magnetism in the space of two or three seconds when kept at a distance from all ferruginous bodies which are capable of attracting it, and of being attracted by it. These facts prove wire of number 32 to be a magnet, the virtue of which is conditional, because its permanency depends on the presence of soft iron, and perhaps on no other circumstance; for the experiment may be repeated with success upon rusty wire of the same size, or on pieces which have been made red hot in the flame of a candle, or surrounded by sand in a crucible, in which situation they will cool much more gradually than when drawn singly through a flame.

Exp. 3. This capacity of iron wire to preserve the magnetism imparted to it, as long as it remains in contact with a bar of the same metal, is a property confined to certain sizes; for let the first experiment be repeated with a small piece of numbers 18 or 17, not equal to half a grain in weight, and just as it comes from the hand of the workman, this piece will perform the part of a particle of soft hammered iron, that is it will drop from the end of the bar, to which it has been attached by the application of a magnet, to the opposite extremity, as soon as the magnetic influence ceases to act upon it; consequently the mere operation of drawing soft iron into wire, by forcing it through a conical hole too narrow for its present diameter, will not convert it into a conditional magnet.

Amongst other experiments relating to the subject, I took the trouble to examine the quality of every size from 32 to 21, both inclusive; the 11 smallest wires, the extremes of which were 32 and 22, were all conditional magnets; that is, they all adhered to the bar of soft iron, to which they had been previously attached, after the removal of the magnet. Number 23 supported seven grains of lead including its own weight, without the assistance of the magnet; No. 21, $6\frac{1}{2}$ nearly; No. 32, $4\frac{1}{2}$; No. 22, no more than two grains.

Wires of low sizes not conditional magnets.

The lowest size capable of conditional magnetism ascertained.

As for number 21, it possessed the simple properties of soft iron: for the shortest cylinder which could be taken from a rod of this size by means of a cutting file, dropped from the end of the horizontal bar as soon as the magnet was withdrawn.

Remarks on
Exp. 3d.

It is difficult to say, which of the 11 wires mentioned above, had the magnetic virtue in the most perfection, because each piece differed in diameter from the rest; besides which, it is very well known, that a mass of iron, of a weight and figure determinable by experiment only, is attracted by any particular magnet, more powerfully than any other mass of the same metal. But the preceding trials have discovered one circumstance apparently of some importance, for they shew that wire is converted into a conditional magnet by its passage through the 22 wordle, or wire drawers instrument; and that the 23d operation brings this quality in it to perfection as far as we can judge from experiment.

I here only speak of wire drawn in Kendal, for I have been told, that the same article manufactured in some parts of Yorkshire, has a much greater propensity to become magnetic. This information was communicated to me by Mr. Morrice, a very intelligent superintendant of a manufacture of cards in this town; who moreover observed, that wire of this description acquires a degree of magnetism under the shears, which induced him, when employed in working it, to substitute a brass gauge for the common instrument made of iron.

Conjectures relative to the
cause.

The magnetic property which commences with number 22, seems to be common to all the finer sizes, for I found it in the smallest wire I could procure, and which apparently did not exceed a strong human hair in thickness.

The foregoing experiments, besides proving that slender wires acquire a magnetism which is permanent as long as they remain in contact with iron, also affords an exception to a second general maxim of the science, which asserts, that the permanency of communicated magnetism depends on the hardness of the ferruginous body that receives it. This does not appear to be the case in experiment 2, in which wire of No. 32 did not lose the faculty of being convertible into a conditional magnet after undergoing a red heat, a process that is well known to render wire very soft. I even repeated the experiment with the same result on all sizes betwixt 22 and

and 33, except 26; pieces of each sort were heated both in the flame of a candle, and in sand; all of which retained the faculty under consideration after being treated in both ways. In reality, wires that had been thus softened, seemed to be in the same condition with small nails of cast iron, considered as retainers of magnetism, though the latter are of a much harder quality; for a nail of the sort called sparrow-bills by shoe-makers exhibited the appearances described in the second experiment, after being filed down to the thickness of a small wire.

If then that kind of magnetism which I have ventured to call conditional do not depend on comparative hardness, to what cause is the phenomenon to be described? little can be offered on my part, besides probable conjecture, in answer to this question. The temperature of wire is considerably raised during its passage through the wordle; and may not we imagine with some shew of reason, that this encrease of temperature, joined to the subsequent contact of cold air, produces a new arrangement of the molecules constituting the wire which enables it to retain a portion of magnetism as long as it remains in contact with a ferruginous body? if this supposition be true, experiment proves the new arrangement to take place in the 22 wordle; when the slenderness of the wire will occasion it to cool suddenly after passing through the instrument. The reality of such changes in the texture of bodies which are not in a state of fusion, is admitted at present by experimental philosophers. I may also quote in favour of this hypothesis some valuable observations made by Gregory Watt, Esq, on the various degrees of magnetism exhibited by the same bazaltic stone under different forms of crystallization; which observations may be seen in your Journal for February, 1805.

Any attempt to explain the permanent magnetism of small wires during their connection with soft iron, and the loss of this property which ensues when the connection is broken, appears to be superfluous, because the fact is evidently analogous to the well known method of adding strength to a magnet by a gradual encrease of its load; for this operation, when judiciously conducted, gives a magnetic charge to a bar of steel already touched, which it cannot retain after the weight is removed.

I remain, &c.

JOHN GOUGH.

P. S. I neg-

P. S. I neglected to mention the following circumstance in the body of the letter. The drawing instrument, or wordle, is made of steel; and is it not probable that this tool, possessing a slight degree of magnetism given to it by friction or otherwise, assists in producing the necessary arrangement, by acting upon heated and slender wires, while their particles are in a violent motion from the pressure of the instrument itself? This supposition has some claim to plausibility; because a weak magnet will impart a portion of the same virtue to a bar of tempered steel, the particles of which are in a state of vibration; for a rod of this metal will acquire a degree of polarity, provided it be struck on the end with a hammer when its axis lies parallel to the dipping-needle.

III.

Concerning the Differences in the magnetic Needle, on Board the Investigator, arising from an Alteration in the Direction of the Ship's Head. By MATTHEW FLINDERS, Esq. Commander of his Majesty's Ship Investigator. From the Philosophical Transactions, 1805.

The magnetic needle is affected at sea by the position of the ship's head:

WHILE surveying along the south coast of New Holland, in 1801 and 1802, I observed a considerable difference in the direction of the magnetic needle, when there was no other apparent cause for it than that of the ship's head being in a different direction. This occasioned much perplexity in laying down the bearings, and in allowing a proper variation upon them, and put me under the necessity of endeavouring to find out some method of correcting or allowing for these differences; for unless this could be done, many errors must unavoidably get admission into the chart. I first removed two guns into the hold, which had stood near the compasses, and afterwards fixed the surveying compass exactly a-midships upon the binnacle, for at first it was occasionally shifted to the weather side as the ship went about; but neither of these two arrangements produced any material effect in remedying the disagreements.

scarcely from the iron on deck.

The following table contains the observations for the variation of the compass in which the differences are most remarkable, and from which I shall beg to point out such inferences as I think may be drawn from them.

TABLE,

TABLE, shewing the Errors produced in the Magnetic Compass by the proper Magnetism of the Ship.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1801. Dec. 9, AM	33° S	116° 28' E	two three theodolite	4 azimuths 6 — 1 —	binnacle on shore —	7° 0' W 6 15 —	5° 59' W 6 23 6 8	NW b. N — —	Commander
1802. Jan. 9, PM	34 1	121 20	one	2 —	binnacle	5 0	9 22	ESE	
16, AM	Goofe Island Bay		one	2 —	—	4 0	0 54	W	
1803. May 20, AM	—	—	—	2 —	—	—	6 8	E	
1802. Jan. 18, PM	33 37	124 10	—	2 —	—	4 30	5 44	NNE	
20, PM	32 33	125 35	—	2 —	—	—	7 15	E b. N	
21, PM	32 30	125 48	—	2 —	—	—	4 45	S	
22, PM	32 30	126 7	—	2 —	—	4 15	6 13	NE b. E	
23, PM	32 21	126 34	—	1 amplitude	—	4 0	4 18	S b. E	
24, AM	32 5	128 15	—	2 azimuths	—	3 0	6 4	E b. N	
26, PM	32 15	128 15	three	6 —	—	—	3 7	S b. E	
30, AM	32 18	132 29	one	2 —	—	0 30	1 41	SSE	Lt. Flinders Commander
Feb. 4, AM	No. 4, bay in island		—	2 —	—	0 15	2 23	Easterly	
5, AM	32 39	133 55	—	1 amplitude	—	—	1 56	E b. S	
6, AM	32 36	133 54	three	6 azimuths	—	—	1 0	NW	Lt. Flinders Commander
26, PM	34 3	135 20	one	2 —	—	1 5	1 33	SE b. E	
—, PM	34 5	135 24	—	1 amplitude	—	1 5	3 56	SW	

TABLE

TABLE, showing the Errors produced in the Magnetic Compass by the proper Magnetism of the Ship.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1802.									
Feb. 18, AM	31° 50' S	135° 32' E	three theodolite	6 azimuths	binnacle on shore	1° 12' E	1° 12' E	S	Lt. Flinders
Mar. 1, PM	In No. 10, bay	—	one	1 —	binnacle	1 39	1 39	—	Commander
5, PM	—	—	—	2 —	—	1 39	0 53	S b. E	
17, PM	34 12	137 20	—	1 amplitude	—	2 15	4 38	SW b. S	
18, PM	34 23	137 36	one	2 azimuths	binnacle	2 15	0 35	SE	
21, AM	35 33	137 15	three	6 —	—	2 40 E	1 10	SE b. S	
23, AM	Kangaroo Island	—	two	4 —	—	2 58	6 31	SSW	
26, AM	35 10	137 41	one	1 amplitude	—	2 45	1 49	NE b. N	
27, AM	35 21	137 52	one	1 —	—	2 50	2 58	SSE	
April 6, AM	Kangaroo Island	—	—	2 azimuths	on shore	2 58	5 11	—	
10, AM	35 47	139 15	—	1 amplitude	binnacle	3 0	0 50	W b. S	
11, PM	35 53	139 26	—	2 azimuths	—	3 0	1 25	SE	
13, PM	36 45	140 5	—	2 —	—	3 30	2 20	SE b. S	
16, AM	37 55	139 55	—	{ 2 — 1 amplitude }	—	4 15	2 2	—	
17, PM	37 57	139 56	—	2 azimuths	—	4 15	11 52	NE	
22, AM	39 38	144 50	—	2 —	—	7 45	3 41	WSW	
26, PM	39 35	141 25	—	2 —	—	7 30	6 48	NE b. E	
—, AM	38 38	144 35	—	1 amplitude	—	7 30		NE b. N	

Note. All the compasses made use of on board the Investigator were of Walker's construction, one excepted, which was made by Adams, and used only on July 22, 1801.

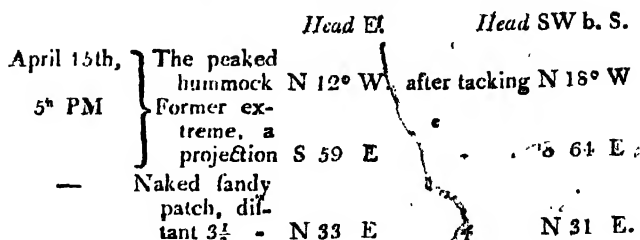
It is apparent that some of the observed variations in the above table are 4° less and others 4° greater than the truth; and it may be remarked, that when this error is westward, the ship's head was east or nearly so, and when it was eastward the head was in the opposite direction. When the observations agree nearest with what was taken on shore, or with what may be deemed the true variations the ship's head was nearly north or south; and a minute inspection of the table will favour the opinion, that the excess or diminution of the variation was generally in proportion as the ship's head inclined on either side from the magnetic meridian.

The errors were about 4° each way, and the north end of the needle deviated as if repelled by the ship's head, &c.

After I had well ascertained the certainty of a difference in the compasses, arising from an alteration in the point steered, I judged it necessary, when I wanted a set of bearings from a point where we tacked the ship, to take one set just before and another immediately after that operation: some specimens of these here follow.

1802.	Head ESE.	Head SW b. W.	Other observations.
April 13th, { Le Geographe			
11 ^h 32' AM { Rocks -	N 55° to 71° E		
— { π point -	N 4 W after tacking	N 9° W	
— { II point -	S 32 E	S 40 E.	
	Head SE b. E.	Head W. •	
April 14th, { II point rocky,			
9 ^h 29' AM { inner part	N 39° E. after tacking	N 30° E	
— { projecting part	N 67 E	N 59 E.	
— { Furthest visible			
— { extreme from			
— { deck	S 51 E	S 55 E.	
	Head ENE.	Head SW b. S.	
April 15th, { W. the western			
11 ^h 50' AM { part	N 15° W. after tacking	N 21° W	
— { A peaked hum-			
— { mock	N 19 E	N 15 E	
— { Furthest extreme			
— { from deck	S 53 E	S 61 E	
— { Centre of a naked			
— { sandy patch	E	E 5 N.	
Variation per amplitude April			
15, AM, taken with the sur-			
veying compass	4° 8' E, ship's head being S.		

April • •



Limits of error
in observing
bearings on
ship-board.

They may
amount to two
or even three
degrees.

Results similar
to those first
stated.

From some little change of place after tacking the ship, and from the part whose bearing was set not being perhaps the individual spot in both instances, the difference between the separate bearings in any set will not be always the same: to these causes for error also may be added inaccuracies in taking the angles arising from the motion of the ship and compass, from the view of the object being obstructed by the rigging, masts, or ship's upper works, and from too much haste to get the bearings before the ship's place was materially altered. Even in the Table of azimuths and amplitudes greater accuracy than one degree must not be looked for; and in ship-bearings two or even three degrees is not, I believe, too great an allowance for error, unless in very favourable circumstances. Without attending to small differences, it is evident that the bearings correspond with the observation in requiring a less east variation to be applied when the ship's head was easterly, and a greater when it was to the westward, in order to get at the true direction of the object *. When examining the north and

* As a specimen of the plan I followed in protracting such bearings as the above, take the set of April 15, A M, when the true variation appears to have been 4° E. On the first bearing the ship's head was six points on one side of the meridian, and on the second it was three points on the other side, the mean is one point and an half on the east side; now for this one point and an half I allow 1° of error, which, as it is on the east side of the meridian, and the variation is easterly, must be subtracted: the variation then to be allowed upon the mean between the bearings before and after tacking will be 3° E, from which the true bearings will stand as follows:

and east coasts of New Holland, I always endeavoured to take the angles on shore with a Troughton's portable theodolite, and to observe for the variation in the same places, that all the errors might be done away or corrected; and as I was frequently fortunate enough to carry on my surveys in this manner for weeks together, instances that might corroborate or contradict the preceding remarks are neither very numerous or pointed; the following are the most remarkable:

April 15th, AM 7	W western part	- - -	N 15° E
11 ^h 50'	A peaked hummock	- - -	N 20° E
—	Furthest extreme from deck	- - -	S 54° E
—	Centre of a naked sandy patch	- - -	E 0 $\frac{1}{2}$ S.

In the same manner upon single sets of bearings I was obliged to allow a variation different from what I supposed the true to be, unless the ship's head was nearly north or south: but, that I might proceed as little upon conjecture as possible, I always endeavoured to get observations for the variation when the ship's head was in the same direction as when I had taken or wished to take a particular set of bearings, and I then allowed that variation exactly, whatever it was. The perplexity arising from disagreements in bearings was by these means much alleviated, and happy agreements were frequently produced, when, without such corrections, there was nothing but discord.

TABLE of observed Variations of the Compaſs, and of the Influence of the Ship's Poſition upon them.

Time.	Latitude.	Longitude.	Number of compaſſes uſed.	Number of ſets of obſervations taken.	Place of the compaſs.	Suppoſed true variation.	Obſerved variation.	Ship's head.	Obſerver.
1802.									
Aug. 5, PM	23° 51' S	151° 42' E	one	1 amplitude	binnacle	5° 0' E	12° 7' E	WSW	Commander
12, PM	23 51	151 40	—	—	—	—	10 15	WNW	Lt. Flinders
12, PM	23 30	151 11	three	6 azimuths	—	—	6 50	SSE	Commander
18, PM	23 23	151 16	one	2 —	—	7 45	7 52	W	
31	22 23	150 38	two	1 —	—	7 30	4 49	E	
Sept. 6, AM	Upon Pier Head		theodolite	1 —	on ſhore	3 0	8 2	—	
Oct. 14, PM	20 44	150 42	one	1 amplitude	binnacle	7 0	6 40	SSE	Lt. Flinders
20, PM	19 22	148 40	—	1 —	—	6 0	5 39	S	Commander
21, AM	18 15	148 38	three	6 azimuths	—	—	5 42	N b. E	Lt. Flinders
Nov. 2, PM	10 30	142 32	one	2 —	—	4 0	3 32	E	Commander
7, AM	12 11	142 0	—	2 —	—	—	4 4	S	Lt. Flinders
9, PM	12 37	142 2	—	1 amplitude	—	—	5 24	W	Commander
1803.									
Jan. 3, PM	14 20	136 16	—	1 —	—	2 30	0 58	E	
7, PM	14 20	136 37	—	1 —	—	—	1 9	SSE	
13, { PM } { AM }	13 38	137 20	—	2 —	—	3 0	3 47	Westerly	Lt. Flinders
14, AM	13 35	136 58	—	1 —	—	—	5 51	WSW	Commander
16, PM	In NW Bay (Gr. Eyl.)		theodolite	1 azimuth	on ſhore	—	3 6	—	
Feb. 3, AM	Arnhem S Bay		three	6 —	binnacle	2 20	2 26	NW b. W	
9, AM	—	—	theodolite	1 —	on ſhore	—	2 20	—	
Mar. 10, AM	11 5	134 15	one	2 —	binnacle	1 0	1 55	WNW	

In the latter of these observations, the differences arising from a change in the direction of the ship's head is less considerable than in the higher latitudes; indeed, on approaching the line of no variation upon the south coast, the differences in the variation were smaller than before and afterwards; but that these differences shall be greater in a large variation and smaller in a less, both places being equally distant from the magnetic pole, I will not venture to assert. The inferences that I think may be safely drawn from the above observations are as follows: 1st. That there was a difference in the direction of the magnetic needle on board the Investigator when the ship's head pointed to the east, and when it was directed westward. 2d. That this difference was easterly when the ship's head was pointed to the west, and westerly when it was east. 3d. That when the ship's head was north or south the needle took the same direction or nearly so that it would on shore; and shewed a variation from the true meridian, which was nearly the medium between what it showed when east and when west. 4th. That the error in variation was nearly proportionate to the number of points which the ship's head was from the north or south. Constant employment upon practice has not allowed me to become much acquainted with theories, but the little information I have upon the subject of magnetism has led me to form some notion concerning the cause of these differences, and although most probably vague and unscientific, I trust for the candour of the learned in submitting it, as well as the inferences above drawn, to their judgment.

1st. I suppose the attractive power of the different bodies in a ship, which are capable of affecting the compass, to be collected into something like a focal point or center of gravity, and that this point is nearly in the center of the ship where the shot are deposited, for here the greatest quantity of iron is collected together.

2d. I suppose this point to be endued with the same kind of attraction as the pole of the hemisphere where the ship is; consequently, in New Holland the south end of the needle would be attracted by it and the north end repelled.

3d. That the attractive power of this point is sufficiently strong in a ship of war to interfere with the action of the magnetic

General inferences. 1. The compass was found to deviate accordingly as the ship's head was easterly or westerly; the north end being farther from the ship's head than the truth. The deviation was proportional to the distance of the head from the N. or S.

Theory proposed. That all the iron in the ship acts like one magnet,

having a different polarity accordingly as the ship is near the N. or S. pole of the terrestrial magnetism.

magnetic poles upon a compass placed upon or in the binnacle.

If these suppositions are consistent with the laws of magnetism, established by experiments, I judge that they will account for all the differences above noticed; for the interference will necessarily be most perceptible upon a compass when the attractive point is at right angles to the magnetic meridian, that is, when the ship's head is east or west, and will altogether vanish or become imperceptible when the attractive point and meridian coincide, or when the ship's head is north

Inferences from the last supposition:

that the effects should have a contrary direction in north latitude.

or south. That the power of this point should become less as the ship increases her distance from the magnetic pole has not indeed entered into my suppositions; but it may probably be true, and is indeed almost a necessary consequence of the second supposition. If the above hypothesis, so to call it, be true, it must follow, that the differences in the variation of the magnetic needle, arising from a change in the ship's head, ought to be directly contrary to those before recited, when the ship is on the north side of the magnetic equator, for the north point of the needle should then be attracted, and the south end repelled. I have no observations which are very decisive upon this head, but those that were taken on board the Investigator seem to bespeak that as it is so; they are as follow.

TABLE of Observations to illustrate the foregoing Inferences.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variations.	Ship's head.	Observer.
1801.									
July 21, PM	Start Point	in sight to the NE.	two	5 azimuths	binnacle	—	29° 34' W	W	Mr. Thistle
—	49° 10' N	5° 25' W	one	1 amplitude	—	—	29 30	—	—
22, PM	—	—	two	4 azimuths	upon the booms in the middle of the ship	—	24 12	WNW	—
—, AM	48 15	6 45	one	1 amplitude	binnacle	—	24 49	WSW	—
28, PM	38 1	14 20	five	10 azimuths	booms	—	20 57	SW	—
—	—	—	—	11	binnacle	—	25 34	—	Commander
31, PM	Porto Santo	in sight to the NW	two	4	—	—	22 45	—	Mr. Thistle
—	—	—	—	4	booms	—	19 51	—	—
Aug. 24, AM	10 20	22 15	one	2	binnacle	—	12 45	SE b. S	Commander
29, AM	5 40	16 30	two	4	—	—	12 18	—	Lt. Flinders
Sept. 5, AM	2 15	14 00	—	3	—	—	14 54	WSW	Mr. Thistle

Remarks and
observations on
the same law.

Instances of the
compass being
affected by local
magnetism.

Pier Head.

Local deviation
of the compass
as far as 50° .

These observations, particularly those of July 28, seem to be decisive in showing that the variation is more westerly when taken upon the binnacle of a ship whose head is westward in north latitude, than when observed in the center of the ship, which is a strong confirmation of the suppositions before given; but the observations on the change of the ship's head are too few to be satisfactory. Almost every sea officer can tell whether he has observed the variation of the compass to be greater when going down the English Channel than when coming up it: and indeed it would be very easy for a ship lying in harbour to ascertain the point beyond controversy. Should this point be well established, I think it would follow, that from a high south latitude where the differences are great on one side, they are most likely to decrease gradually to the equator, and to increase in the same way to a high north latitude, where they are great on the other side; thus the smaller differences on the north coast of New Holland will be accounted for. I shall leave it to the learned on the subject of magnetism to compare the observations here given with those made by others in different parts of the earth, and to form from them an hypothesis that may embrace the whole of the phenomena: the opinion I have ventured to offer is merely the vague conjecture of one who does not profess to understand the subject.

Some account of the magnetism of Pier Head, upon the east coast of New Holland, may not perhaps be thought an inappropriate conclusion to this Paper. I was induced to attend to this from the following passage in Hawkesworth, Vol. III. p. 126. "At sun-rise I went ashore," says Captain Cook, "and climbing a considerable hill," Pier Head, "I took a view of the coast and the islands that lie off it, with their bearings having an azimuth compass with me for that purpose; but I observed that the needle differed very considerably in its position, even to thirty degrees, in some places more, in others less; and once I found it differ from itself no less than two points in the distance of fourteen feet*. I took up some

* In a set of angles taken near the head of Arnhem north bay, on the west side of the gulph of Carpentaria, I found the needle of the theodolite had been drawn 50° from its proper direction. The shore consisted of grains of iron ore caked into a stony mass; and a piece of it, when applied to the needle, drew it six or eight degrees

some of the loose stones that lay upon the ground, and applied them to the needle, but they produced no effect; and I therefore concluded that there was iron ore in the hills, of which I had remarked other indications, both here and in the neighbouring parts."

On landing at Pier Head I found the stones lying on the surface to be porphyry, of a dark bluish colour; but although I understand this species is usually found to possess some magnetic power, a piece did not produce any sensible effect upon the needle of the theodolite when applied to it. In the following observations the theodolite always stood about four feet from the ground, that being nearly the length of its legs. I first took an extensive set of bearings from the top of the hill, amongst which were two stations whence Pier Head had been before set. The first, called Extensive Mount, distant 34 miles, differed from its back bearing $4^{\circ} 35'$ to the right, and the second, island *a*, distant $29\frac{1}{2}$ miles, differed $4^{\circ} 45'$ the same way. I now moved the theodolite three yards to the westward, and the same two objects bore $2^{\circ} 10'$ to the right of their back bearing; on moving it three yards to the south-eastward from the first place, they differed 2° to the left; and on moving the theodolite four yards to the northward, the same two objects bore $1^{\circ} 10'$ to the right of their back bearings. On the following morning I determined to try the magnetism more particularly. Taking the theodolite and dipping-needle, I landed upon the shore of the Head, whence the top of the hill bore $N 50^{\circ} W$, about one-third of a mile. The variation of the

The author's
obs. at Pier
Head.

degrees from its direction, but it then swung back to its error of 50° where it was stationary. In Arnheim south bay a small piece of similar stone drew the needle of the theodolite entirely round, yet the bearings taken in this place did not show any disagreement from the variation and bearings taken in the neighbouring places, where the stone did not produce any such effect. In most places on shore, where I had occasion to take angles, it was my practice to try the effect of a piece of the stone upon the theodolite, in order to detect the presence of iron ore, as well as on account of my survey. It commonly happened that no effect was apparent, but yet I could not trust implicitly to the angles, (particularly on the main land,) unless observations for the variation were taken before the instrument was moved, or I had a back bearing of some station where such observations had been made.

The author's
obs. at Pier
Head.

theodolite in this place I observed to be $8^{\circ} 2' E$, and the inclination of the south end of the dipping needle $50^{\circ} 50'$, the needle stood vertical when the face of the instrument was $S 2^{\circ} E$. I then took the following bearings: Extensive Mount $108^{\circ} 30'$, the same exactly as by back bearing. Double Peak $143^{\circ} 30'$; from hence I rowed round the Head, and landed on a rock, whence the top of the hill bore SSW one-sixth of a mile; Extensive Mount bore $110^{\circ} 14'$, the inclination of the dipping-needle $50^{\circ} 39'$, and the needle stood vertical when the instrument faced $S 3^{\circ} E$. Thus the difference was $1\frac{1}{4}^{\circ}$ in the horizontal, and $\frac{1}{4}^{\circ}$ in the vertical direction of the needle. Ascending the hill, I made the following observations on the top: Extensive Mount $113^{\circ} 50'$, a island $133^{\circ} 52'$, Double Peak $148^{\circ} 32'$; the inclination of the needle was $53^{\circ} 20'$, and it stood vertical at $S 3^{\circ} E$. The differences here are $5^{\circ} 10'$ in the horizontal, and $2^{\circ} 30'$ in the vertical direction, from what the needle stood at in the first morning's place. On moving ten yards SSE, the bearings were, Extensive Mount $109^{\circ} 44'$, Double Peak $143^{\circ} 25'$; the inclination was $52^{\circ} 18'$, and the needle was vertical when the instrument faced $S 5^{\circ} W$. In this 4th set of observations, the horizontal direction of the needle is only a few minutes different from the first place, but the vertical direction is $1^{\circ} 28'$. From the top of the hill I now moved twenty yards to the north-eastward, when Extensive Mount bore 110° , Double Peak $144^{\circ} 42'$; the inclination of the dipping needle was now $50^{\circ} 35'$, and it stood vertical at $S 3^{\circ} W$. Thus it appears that the polarity of the magnetic needle is most interrupted at the top of the hill, both according to the theodolite and dipping-needle. Whether this may arise from some particular magnetic substance lodged in the heart of the hill, or from the attractive powers of all the substances which compose Pier Head being entered in a similar point to what I have supposed to take place with all the ferruginous bodies lodged within a ship, I shall not attempt to decide. The greater differences in the horizontal direction of the needle observed by Captain Cook, might have arisen from his using a common azimuth compass, which was probably not further elevated from the ground than to be placed on a stone.

MATTHEW FLINDERS.

Isle of France,
March 5th, 1804.

Letter

IV.

Letter from Mr. ROBERT HARRUP, shewing that the Smut in Wheat exists in the Seed, and is greatly remedied by Lime sowing.

To Mr. NICHOLSON,

SIR,

Chobham, January 7, 1806.

LITTLE conversant in agricultural affairs, I am yet to learn what enquiries have been made into the nature and causes of the diseases of grain.

If the following communication on the disease of wheat, known by the name of *smut*, contains any thing new, or may lead to farther investigation; an early insertion of it will greatly oblige,

Sir,

Your obedient humble Servant,

R. HARRUP.

Different causes have been assigned for the production of smut; some supposing it to arise from too great an abundance of water shoots, others from intemperate seasons. Causes usually assigned for the smut.

A writer in a respectable publication strenuously contends in favour of the latter opinion.

He informs us, that brine, pickling, liming, change of seed, and seed of one year old, and upwards, avail nothing. In cold wet summers, says he, the smut prevails notwithstanding the use of every means which invention hath urged or ingenuity practised. After a number of observations, he continues, "to sum up the whole of this matter, it seems as certain as demonstration can render it, that the smut is not owing to any defect or imperfection in the seed, but entirely to some corrupt-breating principle in the atmosphere, in the blowing season, which blights and destroys the grain in some shape or other, according to the time it has been blowing, when it is struck with the blight." Intemperate weather: and that attention to the seed avails nothing.

On the contrary, it would appear from the accounts of those who have the most frequent opportunities of making observations, that the primary cause of smut is in the seed. Practical men usually ascribe it to the seed, All the farmers I have conversed with on the subject, are decidedly .

and use prepara-
tions.

decidedly of opinion, that smut in the seed will produce smut in the crop, unless certain means are used to prevent it. With this intention I have somewhere seen a variety of preparations recommended, in some of which arsenic was one of the ingredients. The farmers in this neighbourhood prepare their seed wheat in one or other of the following methods.

Steeping in
brine:

Formerly the wheat was immersed about twelve hours in a strong solution of common salt in water, and afterwards dried by mixing it with a sufficient quantity of lime newly slaked.

wetting,

Of late years, in place of immersing it, they pour a quantity of the saline solution over it on the floor, and after mixing the whole well together, dry it with lime as before.

or treatment
with lime water
only.

Another method which is now pretty generally adopted, is that in which no salt is used.

A quantity of boiling water is poured upon quick lime, and kept constantly stirred till the lime is reduced to powder, when it is immediately mixed with the grain. No great accuracy is used in ascertaining the proportions; five or six pounds of lime, and three gallons of boiling water are about sufficient to prepare five bushels of wheat. In reasoning *a priori*, one would be apt to suppose, that the vegetative powers of the grain would be materially injured by this boiling composition, but experience proves the contrary.

Experiment.
Equal measures
of sound and
smutty wheat
were mixed.
Half of this was
steeped in brine
for twelve hours,
and half left un-
prepared.

Amidst this diversity of opinion on the cause of smut, I wished to ascertain the truth, if possible, by experiment. Accordingly, so early as December 1798, I mixed intimately together equal measures of sound wheat and grains consisting entirely of smut. The heap was then divided into two equal parts; one of them was put into a saturated solution of salt in water for twelve hours, and then mixed with quick lime.

Both parcels
were planted.

The other part was subjected to no preparation whatever. Five or six days afterwards they were both planted in drills on a south border, about nine or ten yards apart. Both parcels came up about the same time, and while in blade, no difference could be perceived. While the ear was yet enveloped by the blade, I cautiously opened several of both crops, and in some of that which had undergone no preparation, a considerable difference was observable. Some of the embryo grains were opened, and in place of a milky juice, they contained only a small quantity of a whitish substance, in which,

by

by the help of a common magnifier, I could readily distinguish from one, to three or four black specks in each. When rubbed between the fingers, a faint smell of smut was emitted. The ears which were examined, and had this appearance were marked, and afterwards proved to be smut. When the crops came out in ear, it was easy to distinguish the smut from the wheat. At the time of blowing no blossom whatever appeared on the smut ears, and the weather proving tempestuous at that time, the blossom was frequently washed off the wheat ears by heavy showers, and as often renewed.

The unprepared seed gave an unhealthy product.

Smut easily distinguished.

Both pieces were cut at the usual time, and upon a careful examination, that which had been subjected to no preparation consisted of nearly two-thirds of smut ears, the remainder being tolerably good wheat. In that which had been prepared, not a single smut-bull could be found.

The prepared seed produced sound grain, and the unprepared seed smutty grain.

An accidental occurrence may be mentioned in corroboration of this experiment. Happening to pass through a small field of wheat just before the commencement of the harvest, I was struck with the unusual quantity of smut in one part of it. On close examination, I found that this extraordinary crop of smut ended abruptly in a line along one of the furrows. The other parts of the field had much the same appearance of others in the neighbourhood; a few smut ears scattered through it. Upon enquiry I found, that the seed with which this field had been sown, running short, the piece so abundant in smut had been sown in seed which contained a considerable quantity of smut, and had undergone no preparation, only sprinkling it with a little slaked lime immediately before sowing.

An accidental occurrence of much smut confined to a part of a field, which had been sown with unprepared seed.

The disease of smut is entirely confined to the grain. The straw and every other part of the plant is sound, and arrives at the natural size. Smut ears are *staring*, and of a dirty whitish colour, inclining to blue, at the time when healthy ears are of a bright yellow. Their odour is *ætid*, and not inaptly compared to that of stale lobsters. Part of an ear is not unfrequently found to contain smut, while the other parts are filled with sound wheat.

Smut affects the grain only.

Description of the smut.

Diseased grains have more the globular form than those of sound wheat, which is perhaps the reason why they are called smut-balls. The skin is shrivelled and of a dirty brownish hue

hue, without any perforations which can be discovered by a high magnifying power. The whole of their contents, in a recent state, are a blackish soft substance with a few shining specks, which disappear when dried.

The dust of
smut consists of
globules,

When kept some time in a dry place, this soft substance is in the form of a fine dust or powder, of a dark brown colour when spread out on glass or talc. The microscope shews each of these minute particles to be well formed globules, somewhat larger than the sanguineous.

heavier than
water :

They are specifically heavier than water with which they readily mix but soon subsides, suffering no change by being kept in that fluid. In the beginning of September last, I infused some of the powder in water in a watch-glass. A few hours after I discovered by the microscope, in a drop of the fluid a few animalculæ. Upon examination next day every drop of the liquor contained innumerable animalculæ, generally very minute but some a size larger. After standing exposed some days, the water evaporated, and an hour or two after the addition of fresh water every part swarmed with animalculæ, moving nimbly in all directions. While viewing them in the microscope they suddenly became motionless owing to the evaporation of the drop of liquid ; on adding a drop of fresh water, they instantly revived and began the same lively motion. A quantity of salt sufficient to saturate the water was then added to the mixture. Upon examination about twenty hours afterwards, I was much surprized to find the animalculæ as numerous and lively as before the addition of the salt.

in which they
produce animal-
cules,

which are not
killed by salt ;

The watch glass with its contents, after standing neglected, on a shelf exposed to the effluvia of a variety of drugs, till the latter end of November, was again filled with water, and placed near a fire, placing at the same time by it a similar glass, containing smut powder and fresh water. They were both frequently examined for some days, but without discovering any animalculæ. My attention being called off by other avocations they remained unnoticed about eight days. The glass which contained the infusion with simple water was quite dry, and only a small quantity of fluid remained in the other. A drop being examined in the microscope by a single lens of a high magnifying power, was found to

to swarm with animalculæ. Both glasses were now filled with fresh water, and placed under inverted jars. Being examined two days after, each of them swarmed with lively animalculæ. While viewing them, a small particle of lime water was added to the drop, which proved instantly fatal, at least all motion ceased instantaneously, and was not renewed.

Lime water killed the animalcules from smut.

Among other inferences which may be drawn from the preceding facts and observations, are first, that the cause of smut is in the seed, and that smut produces smut in the crop. At the same time it is readily admitted, that certain seasons are more favourable to smut than others, which can only be considered as a secondary cause. 2. That lime used in the manner above mentioned, prevents smut, if not entirely, at least so far as not to prove injurious.

Inference. Smut is caused by bad seed, and lime water prevents it.

Is smut occasioned by animalculæ? Some of the foregoing facts seem strongly to favour the idea*.

V.

On the Discovery of Palladium; with Observations on other Substances found with Platina. By WILLIAM HYDE WOLLASTON, M. D. Sec. R. S †.

HAVING some time since purified a large quantity of platina by precipitation, I have had an opportunity of observing various circumstances in the solution of this singular mineral, that have not been noticed by others, and which, I think, cannot fail to be interesting to this Society.

The principal subject of the present memoir is palladium.

* Mr. Nicholson will readily perceive that the subject is not near exhausted. If future investigation should present any thing worthy of communication on the subject, should Mr. N. deem such deserving a place in the Philosophical Journal, he has only to mention it in a marginal line.

Answer. The diseases of corn form a subject of such high importance, whether considered in an economical or scientific point of view, that I must consider it a duty to pay the most marked attention to whatever may tend to elucidate it.—N.

† Philos. Trans. 1805, p. 346.

As

As I have already given an account * of one product obtained from that ore, which I considered as a new metallic substance, and denominated Rhodium, I shall on the present occasion confine myself principally to those processes by which I originally detected, and subsequently obtained another metal, to which I gave the name of *Palladium*, from the planet that had been discovered nearly at the same time by Dr. Olbers.

In the course of my inquiries I have also examined the many impurities that are usually mixed with the grains of platina, but I shall not think it necessary to describe minutely substances which have already been fully examined by others.

§ I. Ore of Iridium.

Ore of Iridium, resembles that of platina, but is insoluble in nitro-muriatic acid; grains,

harder to the file, not malleable and peculiar in their fracture :

much heavier than the grains of platina;

of which metal they contain none.

I must however notice one ore, that I find accompanies the ore of platina, but has passed unobserved from its great resemblance to the grains of platina, and on that account is scarcely to be distinguished or separated from them, excepting by solution of the platina; for the grains of which I speak are wholly insoluble in nitro-muriatic acid. When tried by the file, they are harder than the grains of platina; under the hammer they are not in the least degree malleable; and in the fracture they appear to consist of laminae possessing a peculiar lustre; so that although the greater number of them cannot, as I have before observed, be distinguished from the grains of platina, the laminated structure sometimes occasions an external form by which they may be detected. With a view to be absolutely certain that there exist grains in a natural state, which have not been detached by solution from the substance of the grains of platina, I have separated from the mixed ore as many as enabled me to ascertain their general composition.

Their most remarkable quality is their great specific gravity, which I have found to be as much as 19.5, while that of the crude grains of platina has not, in any experiment that I have made, exceeded 17.7. From this circumstance it might naturally be conjectured that they contain a greater quantity of platina than the grains in general; by analysis, however, they do not appear to me to contain the smallest quantity of that metal, but

* See our Journal, IV. 107.

to be an ore consisting entirely of the metals that were found by Mr. Tennant in the black powder which is extricated by solution from the grains of platina, and which he has called Iridium and Osmium. But, since the specific gravity of these grains so much exceeds that of the powder, which by my experiments has appeared to be, at the utmost, 14.2, I have thought it might deserve inquiry whether their chemical composition is in any respect different. For this purpose I have selected a portion of them, and have requested Mr. Tennant to undertake a comparative examination, from whose well known skill in chemical inquiries, as well as peculiar knowledge of the subject, we have every reason to expect a complete analysis of this ore.

§ II. *Hyacinths.*

Among those bodies which may be separated from the ore of platina, in consequence of their less specific gravity, by a current of water or of air, there may be discerned a small proportion of red crystals so minute, that 100 of the largest I could collect weighed scarcely $\frac{1}{15}$ of a grain. The quantity which I possess is consequently too small for chemical analysis; but their physical properties are such as correspond in every respect with those of the hyacinth. I was first led to compare them with that stone by their specific gravity, which I conjectured to be considerable from their accompanying other substances, that appear to have been collected together solely by reason of their superior weight.

Like the hyacinth, these crystals lose their colour immediately and entirely when heated; they also agree with it in their hardness, which is barely sufficient to scratch quartz, but is decidedly inferior to that of the topaz.

The principal varieties of their form may be very well understood by description.

1st. In its most simple state the crystal may be considered as a rectangular prism terminated by a quadrilateral obtuse pyramid, the sides of which sometimes arise direct from the sides of the prism; but,

2dly. The position of the pyramid is generally such that its sides arise from the angles of the prism. In this case the sides of the prism are hexagons.

3dly. It

3dly. It is more usual for the prism to have eight sides by truncation of each of its angles, and at each extremity eight additional surfaces occupying the place of the eight linear angles between the prism and terminating pyramid of the second variety. The complete crystal has then thirty-two sides.

4thly. The eight surfaces last mentioned, as interposed between the prism and pyramid, are sometimes elongated into a complete acute pyramid having eight sides arising from the angles of an octahedral prism.

which prove the nature of the stone.

The third form above described, corresponds so entirely with that given by the Abbé Haüy** as one of the forms of the hyacinth or jargon, that I have little reason to regret my inability to obtain chemical evidence of the composition of these crystals.

Those, and other impurities, I usually separated, as far as was practicable, by mechanical means, previously to forming the solution of platina, which has been the principal object of my attention.

§ III. Precipitation of Platina.

Account of the treatment of platina. After the first precipitation of it by sal ammoniac: another portion was thrown down by iron.

This is called the first metallic precipitate, and was again dissolved as the first ore had been, and again precipitated by sal ammoniac.

When a considerable quantity of the ore has been dissolved, and I had obtained, in the form of a yellow triple salt, as much of the platina as could be precipitated by sal ammoniac, clean bars of iron were next immersed in the solution for the purpose of precipitating the remainder of the platina.

For distinction it will be convenient to call this, which in fact consists of various metals, the first metallic precipitate. The treatment of this precipitate differed in no respect from that of the original ore. It was dissolved as before, and a portion of platina precipitated by sal ammoniac; but it was observable that the precipitate now obtained was not of so pale a yellow as the preceding. Nevertheless the impurity was in so small quantity, that the platina reduced from it by heat did not differ discernibly from that obtained from the purest yellow precipitate.

* *Traité de Mineralogie*, Pl. XLI. fig. 17. *Journ. des Mines*, No. 26, fig. 9.

At this time I found it advantageous to neutralize the solution with soda, and to employ a solution of green sulphate of iron for the precipitation of the gold, of which, I believe, a portion may always be obtained from the mixed ore; but I have observed in experiments upon any quantities of mere grains of crude platina carefully selected, that the smallest portion of gold could not be detected as a constituent part of the ore itself.

The solution was neutralized with soda; and gold, if present, precipitated by solution of green sulphate of iron.

Bars of iron were subsequently employed as before for recovering the platina that remained dissolved, together with those substances which I have since found to accompany it.

A second metallic precipitate was thrown down by iron.

The precipitate thus obtained, which I distinguish by the name of the second metallic precipitate, was to appearance of a blacker colour than the former, and was a finer powder.

As I was not at first prepared to expect any new bodies, I proceeded to treat the second precipitate, as the former, by solution and precipitation. But I soon observed appearances which I could not explain by supposition of the presence of any known bodies, and was led to form conjectures of future discoveries, which subsequent inquiry has fully confirmed.

When I attempted to dissolve this second metallic precipitate in nitro-muriatic acid, I was surprised to find that a part of it resisted the action of that solvent, notwithstanding any variations in the relative proportions or strength of the acids employed to form the compound, and although the whole of this powder had certainly been twice completely dissolved.

This was not at all soluble in nitro-muriatic acid.

The solution formed in this case was of a peculiarly dark colour, and when I endeavoured to precipitate the platina from it by sal ammoniac, the precipitate obtained was small in quantity, and, instead of being yellow, was of deep red colour, arising from an impurity which I did not at that time understand, but which we since know, from the experiments of Mr. Descotils, is occasioned by the metal now called iridium.

This solution was very dark and its precipitation by sal ammoniac was deep red, occasioned by iridium.

The solution, instead of being rendered pale by the precipitation of the platina, retained its dark colour in consequence of the other metals that remained in solution; but, as I had not then learned the means of separating them from each other, and as the quantity of fluid which accumulated occasioned me some inconvenience, I decomposed it by iron, as

Precipitation of a third metallic precipitate by iron.

in

in the former instances, and formed a third metallic precipitate, which could more commodiously be reserved for subsequent examination.

Much of this being rhodium, was insoluble.

In this last step I committed an error which afterwards occasioned me considerable difficulty, for I found that a great part of this precipitate consisting of rhodium was unexpectedly rendered insoluble by this treatment, and resembled the residuum of the second metallic precipitate abovementioned.

As I have already communicated to this society, in my Paper upon rhodium, the process by which I subsequently avoided this difficulty, I shall at present return to a previous stage of my progress, and relate the means by which I first obtained palladium in my attempts to analyze the second metallic precipitate.

§ IV. *Separation of Palladium.*

Separation of palladium. The second metallic precipitate contained lead, copper, and another metal precipitable by copper.

There was no difficulty in ascertaining the presence of lead as one of the ingredients of this precipitate, by means of muriatic acid, which dissolved lead and iron and a small quantity of copper. It was equally easy to obtain a larger portion of copper by dilute nitrous acid, with which it formed as usual a blue solution. But when I endeavoured to extract the whole of the copper by a stronger acid, it was evident, from the dark brown colour of the solution, that some other metallic ingredient had also been dissolved. I at first ascribed this colour to iron; but, when I considered that this substance had been more slowly acted upon than copper, I relinquished that hypothesis, and endeavouring to precipitate a portion of it by a clean plate of copper, I obtained a black powder adhering to a surface of platina on which I had placed the solution. As this precipitate was soluble in nitric acid, it evidently consisted neither of gold nor platina; as the solution in that acid was of a red colour, the metal could not be either silver or mercury; and as the precipitation of it by copper excluded the supposition of all other known metals, I had reason to suspect the presence of some new body, but was not fully satisfied of its existence until I attempted the precipitation of it by mercury.

It was separated by agitating mercury with the solution with

For this purpose I agitated a small quantity of mercury in the nitrous solution previously warmed, and observed the mercury to acquire the consistence of an amalgam. After this

amalgam

amalgam had been exposed to a red heat, there remained a white metal, which could not be fused before the blowpipe. It gave a red solution as before in nitrous acid; it was not precipitated by sal ammoniac, or by nitre; but by prussiate of potash it gave a yellow or orange precipitate; and in the order of its affinities it was precipitated by mercury but not by silver.

which it formed an amalgam; and the mercury was driven off by heat. It was palladium.

These are the properties by which I originally distinguished palladium; and by the assistance of these properties I obtained a sufficient quantity for investigating its nature more fully.

There were, however, various reasons which induced me to relinquish the original process of solution in nitrous acid and precipitation by mercury; for although I found the metal thus obtained to be nearly pure, the necessity of agitating the solution with the mercury was very tedious, and the waste was also considerable; for in the first place it seemed that nitrous acid would not extract all the palladium from any quantity of the second metallic precipitate, neither would mercury reduce the whole of what was so dissolved. I therefore substituted a process dependent on another of its properties, I had observed that this metal differed from platina in not being precipitated from nitro-muriatic acid by nitre or by other salts containing potash; for although a triple salt is thus formed, this salt is extremely soluble, while that of platina, on the contrary requires a large quantity of water for its solution. On that account a compound *menstruum* consisting of nitrate of potash dissolved in muriatic acid is unfit for the solution of platina, but dissolves palladium nearly as well as common nitro-muriatic acid in which there is no potash present *.

The process with mercury was abandoned;

In five ounces of muriatic acid diluted with an equal quantity of water, I dissolved one ounce of nitre, and formed a solvent for palladium that possesses little power of acting on platina, so that by digesting any quantity of the second metallic precipitate till there appeared to be no farther action, I procured a solution from which by due evaporation were formed crystals of a triple salt, consisting of palladium combined with muriatic acid and potash. These are the crystals which I have on a

and a solvent consisting of muriatic acid, with nitre was used to the second precipitation from which it takes palladium but not platina.

* I have found that gold may also be dissolved with equal facility by the same solvent, and nearly in the same proportion. Ten grains of nitre added to a proper quantity of muriatic acid are sufficient for sixteen grains of either gold or palladium.

The solution gave crystals of triple salt of palladium potash and muriatic acid.

former

former occasion * mentioned as exhibiting a very singular contrast of colours, being bright green when seen transversely, but red in the direction of their axis; the general aspect, however, of large crystals is dark brown.

From the salt thus formed and purified by a second crystallization, the metal may be precipitated nearly pure by iron or by zinc, or it may be rendered so by subsequent digestion in muriatic acid.

§ V. *Reasons for thinking Palladium a simple Metal.*

That palladium is a simple metal appears from,

its forming a distinctly crystallized salt with bases and an acid,

its combinations with metals and separation without change.

and its precipitation is reducible by mere heat.

Q. Whether it might consist of metal and a fixed acid?

From the consideration of this salt alone I thought it highly probable that the substance combined in it with muriatic acid of potash was a simple metal, for I know of no instance in chemistry of a distinctly crystallized salt containing more than two bases combined with one acid. I nevertheless endeavoured by a suitable course of experiments to obviate all probable objections.

After examining by what acids it might be dissolved and by what reagents it might be precipitated, I combined it with various metals, with platinum, with gold, with silver, with copper, and with lead; and when I had recovered it from its alloys so formed, I ascertained that, after every mode of trial it still retained its characteristic properties, being soluble in nitrous acid, and precipitable from thence by mercury, by green sulphate of iron, by muriate of tin, by prussiate of potash, by each of the pure alkalis, and hydrosulphurets.

The precipitate obtained in each case was also found to be reducible by mere heat to a white metal, that, except in very small quantities, could not be fused alone by the blowpipe, but could very readily be fused with sulphur, with arsenic, or with phosphorus, and in all other respects resembled the original metal.

The only hypothesis, on which I thought it possible that I could be deceived, arose from the recollection of the error, which subsisted for a few years, respecting the compound formerly called siderite. It was possible that some metallic or other fixed acid might unite too intimately with either a known or an unknown metal to be separated by the more common simple affinities. I consequently made such attempts as appeared best calculated to disunite a compound so constituted.

* Phil. Trans. 1804, p. 478.

Having boiled the oxide with pure alkalis, and found it to be unaltered, I thought the affinities of lime or lead might be more likely to detect the presence of the phosphoric or of any known metallic acid; and accordingly I made various attempts by muriate and nitrate of lime, as well as by nitrate of lead, to effect a decomposition of the supposed compound. In the experiment on which I placed the greatest reliance, I poured liquid muriate of lime into a solution of palladium in nitromuriatic acid, and evaporated the mixture to dryness, intending thereby to expel any excess of acid that might have been left in the solution, and to render either phosphate of lime, or any compound of lime with a metallic acid, insoluble in water. The residuum however was very readily dissolved by water, and consisted merely of muriate of lime and muriate of palladium, without any appearance of decomposition.

The oxide is not affected by boiling with alkalis;

nor by pouring muriate of lime into its solution.

When I found all my endeavours directed to that end wholly unsuccessful, I no longer entertained any doubt of this substance being a new simple metal, and accordingly published a concise delineation of its character; but by not directing the attention of chemists to the substance from which it had been extracted, I reserved to myself an opportunity of examining more at leisure many anomalous phenomena, that had occurred to me in the analysis of platina, which I was at a loss to explain, until I had learned to distinguish those peculiarities, that I afterwards found to arise from the presence of rhodium.

Hence the discoverer was warranted in publishing it as a new metal.

§ VI. *Additional Properties of Palladium.*

In my former Paper on that subject I also added some observations upon the properties and origin of palladium, describing only such a mode of obtaining it from platina as should avoid the introduction of any unnecessary ingredient which might possibly be misinterpreted, and omitted one of the most distinguishing properties of palladium, by means of which it may be obtained with the utmost facility by any one who possesses a sufficient quantity of the ore of platina.

Method of easily separating palladium.

To a solution of crude platina, whether rendered neutral by Prussiate of mercury, or saturated by addition of potash, of soda, or ammonia, by lime or magnesia, by mercury, by copper, or by iron, and also whether the platina has or has not been precipitated from the solution by sal ammoniac, it is merely necessary to add a solution of prussiate of mercury, for

Prussiate of mercury added to the solution of crude platina, throws down the pure prussiate of palladium;

Heat disengages the pure metal which is not more than one twohundred. part of the original ore.

the precipitation of the palladium. Generally for a few seconds, and sometimes for a few minutes, there will be no appearance of any precipitate; but in a short time the whole solution becomes slightly turbid, and a flocculent precipitate is gradually formed, of a pale yellowish-white colour. This precipitate consists wholly of prussiate of palladium, and when heated will be found to yield that metal in a pure state, amounting to about four or five tenths *per cent.* upon the quantity of ore dissolved.

More mercury does not augment the product.

The prussiate of mercury is peculiarly adapted to the precipitation of palladium, exclusive of all other metals, on account of the great affinity of mercury for the prussic acid, which in this case prevents the precipitation of iron or copper; but the proportion of mercury does not by any means influence the quantity of palladium, for I have in vain endeavoured, in the above experiment on crude platina, to obtain a larger quantity of palladium than I have stated by using more of the prussiate of mercury, or to procure any precipitate by the same means from a solution of pure platina.

The decomposition is by double affinity.

The decomposition of muriate of palladium by prussiate of mercury is not effected solely by the superior affinity of mercury for the muriatic acid, but is assisted also by the greater affinity of prussic acid for palladium; for I have found that prussiate of palladium may be formed by boiling a precipitated oxide of palladium in a solution of prussiate of mercury.

Prussiate of mercury is the test of palladium.

The prussiate of mercury is consequently a test by which the presence of palladium may be detected in any of its solutions; but it may be worth observing, that the precipitate obtained has not in all cases the same properties. In general, this compound is affected by heat similarly to other prussiates, but when the palladium has been dissolved in nitrous acid and precipitated from a neutral solution by prussiate of mercury, the precipitate thus formed has the property of detonating when heated. The noise is similar to that occasioned by firing an equal quantity of gunpowder, and accordingly the explosion is attended with no marks of violence unless occasioned by close confinement. The heat requisite for this purpose is barely sufficient to melt bismuth, consequently is about 500° of Fahrenheit. The light produced is proportionally feeble, and can only be seen in the absence of all other light.

The precipitation from a nitrous solution detonates by low heat.

In endeavouring to dissolve a piece of palladium in strong colourless nitric acid for the purpose of forming the detonating prussiate, I found that, although the acid shortly acquired a red colour surrounding the metal, the action of the acid was extremely slow, and I was surprised to observe a fact that appears to me wholly singular; the metal was taken up without any extrication of nitrous gas; and this seemed to be the cause of the slow solution of this metal, as there was not that circulation of this fluid, which takes place in the solution of other metals until the acid is nearly saturated.

Palladium is very slowly acted on by nitric acid and extricates no gas.

As the want of production of gas appeared to retard the solution of palladium, I tried the effect of impregnating a quantity of the same acid previously with nitrous gas, and observed its action to be very considerably augmented, although the experiment was necessarily tried in the cold, because the gas would have been expelled by the application of heat.

Nitrous acid acts more strongly.

Beside those properties which are peculiar to palladium there are others, not less remarkable, which it possesses in common with platina. I have on a former occasion mentioned that these metals resemble each other in destroying the colour of a large quantity of gold. Their resemblance, however, in other properties is not less remarkable, more especially in the little power they possess of conducting heat, and in the small degree of expansion to which they are liable when heated.

For the purpose of making a comparison of the conducting power of different metals, I endeavoured to employ them in such a manner, that the same weight of each metal might expose the same extent of surface. With that view I selected pieces of silver, of copper, of palladium, and platina, which had been laminated so thin as to weigh each 10 grains to the square inch. Of these I cut slips $\frac{1}{8}$ of an inch in breadth, and four inches long; and having covered their surfaces with wax, I heated one extremity so as to be visibly red, and, observing the distance to which the wax was melted, I found that upon the silver it had melted as far as $3\frac{1}{2}$ inches: upon the copper $2\frac{1}{2}$ inches: but upon the palladium and upon the platina only one inch each: a difference sufficient to establish the peculiarity of these metals, although the conducting power cannot be said to be simply in proportion to those distances.

Conducting powers of palladium and platina as to heat, tried by the melting of wax upon them. The measure is not half those of silver and copper.

In order to form some estimate of the comparative rate of expansion of these metals, I rivetted together two thin plates of platina like together.

Rate of expansion by heat, tried by rivetting platina like together.

While steel expands through 12, platina will expand 9, and palladium 10.

platina and of palladium; and observing that the compound plate, when heated, became concave on the side of the platina, I ascertained that the expansion of palladium is in some degree the greater of the two. By a similar mode of comparison I found that palladium expands considerably less than steel by heat; so that if the expansion of platina between the temperatures of freezing and boiling water be estimated at 9 parts in 10,000, while that of steel is known to be about 12, the expansion of palladium will probably not be much more or less than 10, or one part in 1000 by the same difference of temperature.

It must, however, be acknowledged, that the method I have pursued is by no means sufficient for determining the precise quantity of expansion of any substance; but I have not been induced to bestow much time on such an inquiry, since the extreme scarcity of palladium precludes all chance of any practical utility to be derived from a more accurate investigation.

VI.

*Report made to the Athénée des Arts of Paris, by MM. RONDELET, BRAUVALLET, and DUCHESNE; on the founding the Statue of JOAN OF ARC in Bronze, by a Way never before used for large Works, by MM. ROUSSEAU and GENON, under the Direction of M. GOIS, Statuary.**

Casting in sand used hitherto only for small figures.

THE method of casting in sand hitherto has only been used for figures from 63 to 70 centimetres (about 2½ feet) in height; while the statue, which was to be formed, being of much larger dimensions, should of course be managed according to the method called the *great foundry*, on account of its being used for colossal statues.

The great foundry known to the ancients, but was lost.

At its revival large statues were cast in separate parts.

This method of casting was known to the ancients, who were even superior to us in it; but this art was lost with many others, and in the time of the Medicis large statues were not formed at a single casting. The figures of Henry the Fourth and of Lewis the Thirteenth, which are seen at Paris, were

* *Magazin Encyclopédique*, T. I. p. 350.

placed

placed on horses made previously, one for the statue of Ferdinand, Grand Duke of Tuscany, and the other for that of Henry the Second, King of France.

The statue of Lewis the Fourteenth, in the Place de Vendôme, is the first that was formed at a single casting since the revival of the art. It was suitable to so great a prince to permit his image to be made solely by a grand method; but Girardon and Keller, to whom the work was entrusted, then made their first attempts, which occasioned many faults, such as the casting it too thick, which in uselessly employing more metal, increased the difficulty of supporting the colossal figure; and such also as using unnecessary labour; but notwithstanding all their precautions, the casting did not succeed perfectly, and considerable repairs were obliged to be made in it. That of Lewis XIV. the first cast after this in a single piece.

About the same time were erected the equestrian statues of this prince, at Bouffers and at Lyons, by the same Girardon, at Rennes by Coizevox, at Montpellier by Mazeline and Utreis, and at Dijon by Le Hongre. Various other statues made in the same way.

After this, Le Moine had to found a statue of Lewis the Fifteenth at Bourdeaux, which met with great accidents; but he had more success with one at Rennes, which was a pedestrian statue: Guibal also made one for Nancy. But this art did not attain to a great perfection till Bouchardon was employed to construct an equestrian statue of Lewis the Fifteenth at Paris: The great care of M. Goor prevented any accident from happening to it, or to that also which was cast at Reims by the same artist, from the model of Pigale. This founder had not the same success when he formed the statue of Frederic the Fifth at Copenhagen, from the model of Saly, which required great repairs. Finally, great improvements had been made in the art when the statue of Peter the Great was founded at Petersburg by Falconet, and nevertheless he was obliged to refound a second time the upper half of the statue. The art not very perfect before the statue of Lewis XV. was cast.

The great disadvantage of the method hitherto used, is its enormous expence and the great time it requires. It is true, that for works which are intended for duration economy is not the chief object; but if they can be performed equally well by M. Gois's method, at one-half the expence and in a fourth of the time, it is in various respects superior to the great foundery. Great improvements made in the art when the statue of Peter the Great was cast.

M. Gois's method superior to it in various respects.

Reasons for believing that it would answer for the largest works.

of the time, it certainly ought to be preferred. There is good reason also to decide, that this method will do equally well for the largest works; for, according to calculation, the largest statue of this kind in France exceeds that of Joan of Arc by a much smaller proportion than the latter exceeds that of the largest statues ever before cast in the same manner, which never weighed more than from 8 to 10 kilogrammes (from about 17 to 22 lb.) The statue of Joan of Arc weighed 600 kilogrammes, which is 60 times more; but that of Lewis the Fifteenth, which weighed 17,000 kilogrammes, was only twenty-eight times heavier than that of Joan of Arc.

But in order to judge better of the advantages of M. Gois's method, it shall be described at large, and an account given also of the method of molding by wax, or of the *grand foundery*, in order to compare them together.

M. Gois's statue of Joan of Arc exhibited and admired.

M. Gois having made a statue of Joan of Arc for a prize, exhibited it in public in the year 10, (1802.) The prefect of the department of the Loire saw it, and proposed to the city of Orleans to re-erect that monument to the glory of this heroine, which had been destroyed in the anarchy of the revolution. It was accordingly ordered to be done. M. Gois being informed of this, went to Orleans and offered to make a cast in bronze from his statue, without precisely knowing whether it was that which the city required.

He is employed to make a cast in bronze from it for the city of Orleans.

An agreement was then made with M. Gois to complete the statue at a fixed price, in the course of about one year from the 5th Germinal, *An XI.* or before May 4, 1804.

Is induced to have the cast made in sand to save time and expence.

M. Gois began to be alarmed at the enormous expence of the usual method of casting such statues, and at the great time it required, which he feared would prevent his performing the agreement. He knew that M. Rousseau had made a cast from the groupe of Graces by Germain Pilon, with great success, by a different method; and though these figures were but 1,38 metres high, and his statue was more than two metres, ($6\frac{1}{2}$ feet) he went notwithstanding to consult this founder, who engaged for the success of the method, and promised to employ in the business the same workman who had cast the above groupe without having met with any accident: This last consideration determined M. Gois to entrust the work to the *founders in fund*.

M. Rousseau undertakes the work for him, who had before made a fine cast of the Graces; and employs the same workman who cast them.

The

The first of Fructidor, *An XI.* the business was began; but as they commenced with the bas-reliefs, it was not till three months after that they undertook the work of the statue.

They made use of the common sand of the founders, which is argillaceous, and always kept a little moist. After having well raked it, separated all the stones, and broken all the lumps that could be met with, they filled with it a case of 2,20 metres long, and one metre broad at the inside, and 16 centimetres high; the thickness of the wood of the case was eight centimetres. The sand was strongly beaten with a rammer 10 centimetres broad and 60 long, and by this operation acquired sufficient consistence to be raised along with the case without any danger of running out.

After this the statue was placed upon the first case, which is called the *false mold*, because it was to be afterwards replaced by another: the sand was stirred up a little, to permit the most prominent parts to enter it; another case of the same size was then put over the first, and attached to it by four points of iron.

The true *concave mold* was then began, by modelling each part of the figure with the same kind of sand. A workman of much address and intelligence is requisite for the division and distribution of the different pieces which form the mold: he should explain the motives which induced him to prefer one distribution to another: each piece should have different sections: care should be taken to mold the parts which have a large and uniform surface in a single piece, while the pieces must be multiplied for those portions of the statue which have many sinuosities and deep indentations.

This part of the operation requires the most care; for if it be performed with negligence, the extraction of the model would be attended with great difficulties; and if the workman employed is awkward, numerous faults will need reparation after the casting, and probably great accidents may happen. It is but justice to say, that Genon, the workman on this statue, shewed in his performance equal dexterity and knowledge.

To prevent the pieces of the mold from adhering to each other, care is taken to powder the parts of each which is finished, with charcoal dust inclosed in a bag, before a new piece is began. The workman having finished the mold-

ings •

ings of the contours of the figure, filled up the empty spaces between them and the case with sand, which he first pressed and forced together with his hands, afterwards beat it with the bat, and finally with a mallet; this compression gives it such a solidity that it appears like stone, or at least like baked earth.

The same care was taken with each case as they were successively added, to the number of seven; which completing the top of the statue, the whole was then reversed in order to replace the lowest case, which, as mentioned, was only a false mold; and then each part at the lower extremity was also modelled in same manner as the preceding.

The hollow mold being finished, the cases were taken asunder, and each piece removed separately to take out the statue; then they were all placed in their proper order in the exterior mold, which may be compared to the cover used by those who make plaster-of-Paris casts. Each piece would be well retained in its place by its irregular form; but it was still farther fastened by a little thin paste made of flour, which was applied by a brush both to the pieces themselves and the parts that adhered to the cases. It was thought necessary to take a precaution more than what was usual, through the apprehension that the paste would not hold together those large pieces as well as it did the small pieces in lesser works; they were therefore traversed by long wires of iron, which entered into the cover or exterior mold.

The pieces of the mold cemented together with paste, and secured by wires passing into the cases.

Method of forming the core.

This mold being thus entirely completed, had only to be dried till the time of the casting. A new mold was necessary to be made to cast the core: the same pains were not taken with this as with the first, as it would be useless to do so. When this second mold was finished, a coat of modelling-clay was applied to its inside, of the same thickness which was intended to be given to the bronze; and without waiting for its drying, it was closed and the core cast in it, which was composed, as is usual, of equal parts of plaster of Paris and of brick dust.

Eight iron rods laid to as to project a few inches from the core, in order to place it in the mold.

Eight rods of iron having been placed at the inside of the mold, afterwards projected from the core about 10 or 12 centimetres, which served to place it with precision in the hollow mold.

But

But in the mean time the cases had been placed one over the other, and the iron pins which connected them fitted to their places, taking care to divide them into two portions, which answered to the two cases usually employed by the founders; and which, instead of the usual thickness of five centimetres at most, were, the one 48 and the other 64 centimetres thick.

In this state they were dried, by placing them round a brazier of kindled charcoal, the fire of which had the more power from the mold being divided into two portions, and empty.

The core was likewise dried by placing it over a brazier of charcoal; the same was also placed round it; and in eight hours the moisture was entirely evaporated. It was left to cool, and it was placed in one half of the mold; the second half of the mold was afterwards fitted on, and the whole compressed together by iron presses in the usual manner.

After this there only remained to construct the bason, (*l'échino*), to fuse the metal, and make the cast. These operations being the same for both methods of founding, shall be related, after first as briefly as possible describing the method of casting by the great foundery in which wax is used.

The first operation for the great foundery is to dig a trench proportionate to the size of the figure to be cast, and to surround it with a wall to prevent the earth from tumbling in. After the model is finished it is oiled, and a mold formed from it with plaster of Paris in the usual way, and with the precautions before directed for molding in sand: In each piece of this mold rods of iron are inserted, by which they may be easily lifted when the mold is taken asunder or put up; each of these pieces is numbered, that its proper place may be known.

After this several layers are applied with a brush to the inside of the pieces, of a composition made of 7-tenths of yellow wax, 1-tenth of turpentine, 1-tenth of white pitch, and 1-tenth of hog's-lard, which is melted slowly to prevent its forming bubbles.

When the different layers form a thickness of three or four millimetres, (0.15 inch) cakes of wax are placed inside in those parts where the bronze should be of a greater thickness, and fastenings of sheet-brass are inserted, which may take hold of the core and prevent the wax from falling off.

The pieces of the mold dried.

The core also dried.

The mold and core put together ready for casting, and secured by presses.

Description of casting by the great foundery.

A mold formed from the statue in separate pieces.

A composition of wax laid on to the inside of the mold, of the thickness intended for the bronze.

The

The core cast in this mold, and the modeling of the wax finished by the statuary after it is taken out.

The mold is then fitted together, and the core cast with quickness, that it may form an entire mass, and not lie in layers: As soon as it is solid the model is taken asunder, and the statuary repairs the wax, takes off all the futures of the molds, rectifies the errors which may have occurred, and gives to the work all the perfection of which it is susceptible.

The true mold formed over the wax; its composition

After this the true mold is formed of materials capable of supporting the heat and the impulse of the metal; to compose which three-sixths of earth are mixed with one-sixth of horse-dung, and left to rot in a ditch for one winter: when this mixture is taken out, two-sixths of broken crucibles, well pounded and passed through a sieve, are added: the whole is tempered with urine and beat up on a stone: it is then what is called *potee*.

Formation of the true mold continued.

When it is to be used, a sufficient quantity is taken and water enough added to it, to make it capable of being laid on with a brush; forty coats of it are then laid over the wax successively, (care being taken that one coat is dry before another is laid on), which altogether form a thickness of five centimetres (2 inches.) The mold is then surrounded with flat bands of iron, which cross each other like net-work; then, after rendering the *potee* thicker, by adding earth to it, and hair that has been well beaten, it is laid on over the former work with the fingers, until the mold has attained the thickness of twenty centimetres below and sixteen above (6 and 8 inches); after which it is surrounded a second time with bands of flat iron.

Preparations for, and method of melting out the wax.

After this a wall is built round it, the passages necessary for the fire constructed, and the intervening spaces are filled up with broken bricks: Then the fire is kindled in the passages most distant from the figure, and is gradually increased for nine days, and again diminished for the same space of time. On the second day the wax begins to flow, and continues to do so for ten or twelve days; about half of it is lost.

The mold buried, and the pipes and vents placed ready for raising.

When the fire is extinguished, the work is left some days to cool; then the broken bricks are removed, and before the mold is buried, a coat of plaster, about half an inch thick, is put over it, which is called the *chemise*. Then they proceed to bury the mold, or inclose it with earth, first taking care to stop all the ways through which the wax flowed, and to raise the

the pipes for the vents and for the entrance of the fused metal. The earth used for enclosing the mold should be first screened, and then laid on equally in the excavation. After each course is raised to a thickness of thirty centimetres, (1 foot) it is beaten down till it is reduced to ten.

After this there only remains to build the basin for the reception of the metal, called the *echino*.

In enumerating the operations necessary for the method of casting in sand first mentioned, the authors of the report state them to amount to ten; while those used for casting in the large way last recited, in which wax is used, amount to no less than twenty-eight, each of which they particularize; but as these operations may readily be counted from the relation already given, this catalogue is not inserted here. They also remark, that the laying on the wax on the pieces of the mold takes up much time, as does likewise the preparation of the *potée*: that in the first method the circling with bands of iron is entirely avoided, and the building of the passages for the fire, which are very expensive: that likewise the molding and setting up of the vast number of pipes and vents is saved in it; and in the drying of the work the economizing of fuel is greatly in favour of the first method, for in it these operations are performed in a short time with a very small fire, which in the other method require at least three weeks and a powerful heat: that in the repairing of the wax the statuary must work with his own hands: and that in taking out the statue when cast, there is vastly less trouble in the first method.

The authors here describe the method of erecting the furnace for fusing the metal for the statue of Joan of Arc; but as it was constructed to burn wood, which fuel is not used in our founderies, and as the description would be on other accounts of but little benefit to our artists, it is omitted.

It is only necessary to state, that the place which contained the fused metal was at such an elevation, that, when the stopper which retained it was driven in, it might flow freely into the *echino* through the passage prepared for it.

The mold for the statue was partly buried in the earth, so as to allow a fall for the metal of eight centimetres (3 in.) from the hearth to the entrance of the pipes; and the authors observe, that the trouble of burying the mold might be avoided by lay-

Enumeration of the various advantages of casting in sand, and disadvantages of the great foundery.

The mold of the statue of Joan of Arc laid in the earth ready for casting.

ing

ing it on its side, for which position they think it was sufficiently well prepared.

Parts of the statue cast separate from the rest.

The statue was all formed in one mold, except the skirts, (*pleinte*), one arm, and the plumes of the helmet, which were placed in a separate case: This might have been dispensed with, but it was apprehended that, if they remained with the statue, they would have much encreased the difficulty of the work, by adding to the elevation of the figure.

1000 kilograms of metal fused.

Every thing being prepared for the casting, about 1000 kilograms (about 32 C.) of the metal was placed in the furnace, one half of which metal consisted of old cannon, a third of copper, and the rest of brass; and on the 8th of Germinal, *An XII.* (29th March, 1804), at eight o'clock in the afternoon, the metal, after five hours heating, being in compleat fusion, and the *echino* and the stoppers which closed the two passages for the metal being previously heated, M. Rousseau forced in the plug that retained the metal in the furnace; it flowed immediately into the *echino*; the stoppers were removed from the passages for the metal from thence, and in less than two minutes a little of the metal appeared at the vents, and shewed that the cast was completed.

The cast made by M. Rousseau: It succeeded completely.

The statue requires no repairs or additions.

On removing the sand it was found that no accident had happened but a slight flaw on the stomach of the figure; that the head was quite perfect; and that there had been no partial casting, or any part of the figure necessary to replace, which often happens in the other method.

The reporters recommend the *Athénée des Arts* to give medals to MM. Rousseau and Genon, and to make honourable mention of M. Gois.

The reporters conclude with high encomiums on the advantages of this method of casting, and recommend that medals be given to MM. Rousseau and Genon by the *Athénée des Arts*, in testimony of their merit; and that as the rules of the Society prevented this recompence from being granted to any of its members, honourable mention should be made of M. Gois.

VII.

*Experiments made at the Galvanic Society of Paris, by M. RIF-
FANT, Director of the Nitre and Gunpowder Works, tending
to prove that Muriatic Acid is not composed as announced
by M. Pachiani*.*

AS soon as the Galvanic Society knew that M. Pachiani, The galvanic Society engage in experiments to determine the truth of Pachiani's position.
of Pisa, had announced, that he had obtained muriatic acid by depriving water of a portion of its oxygen, their first care was to engage in a course of experiments both by galvanism and electricity, to obtain, if it was possible, a confirmation of a discovery so important to the progress of science. The society had a letter communicated to them, which was addressed on the 9th of May, 1805, by M. Pachiani, to M. Pignotti; in which he recited the results which he had obtained, but without entering into any detail relative to the nature or order of his experiments; they only knew that he used the galvanic pile. They therefore determined to make their experiments with the same apparatus, in the manner which appeared to them the least likely to produce results liable to objections. Two of these experiments, which appeared principally worthy of attention, were conducted as follows:

Experiment I.

A portion of a new glass tube was taken, three inches long, and 0.35 inch in diameter inside, one of the ends of this tube was closed at the lamp; to the other end was united a capillary tube bent in such a manner as to pass under a jar, and at equal distances from the junction of the capillary tube, two points were drawn out at the lamp in the thickness of the glass, by means of which two bits of gold wire of about 0.02 inch in diameter, and of the standard 0.976 of purity, were inserted in the tube, at a small distance from its lower extremity, and disposed so as not to touch each other, or bear against the sides of the tube; these points of the glass were then closed at the lamp. The tube and its capillary prolongation were filled with distilled water, whose

Description of
the apparatus
used in Exp. 1st,

* *Journal de Physique*, Tom. LXI. p. 281.

purity

purity had been proved by nitrate of silver; the whole was fastened by wax on a small piece of glass, placed in the midst of an horizontal galvanic pile of fifty-two double square plates of 4.25 inches at each side. These plates were separated by pieces of leather, which formed between each other divisions, which were filled with very pure sand, moist-

The sand of the pile moistened with solution of muriate of soda: gas immediately disengaged on its completion,

ened by a solution of muriate of soda. The capillary tube was passed beneath the water of a cistern, with its extremity under the mouth of a jar filled with water. The two wires of gold being made to communicate with the two poles of the pile, its activity was immediately exhibited by the disengagement of gas in chains of bubbles very apparent, parting from the extremity of each of the gold wires, but in a more considerable quantity from that corresponding with the copper pole. This activity of disengagement of gas continued without interruption from the eighth of Thermidor, to the 23d of the same month, on which day, after the pile being moistened with solution of muriate of soda, it stopped for some time: it soon however recommenced, and always did so after any suspension. Its activity was immediately renewed by agitating the wires which communicated with the poles of the pile. The activity of the pile was constantly greatest at four o'clock in the afternoon; and immediately afterwards it began to diminish. On the 11th of Fructidor the apparatus was taken asunder, after continuing for thirty-four successive days in continual action. The water was then diminished to half its original volume, but had not lost any of its limpidity.

the activity of the disengagement of gas renewed by agitating the wires. This activity, always greatest at four in the afternoon.

The apparatus separated after continuing in action thirty-four days.

The water in the tube diminished one half, the gold wires oxidized, that next the zinc most, 793 cubic centimeters of gas collected.

The remaining liquor in the tube has no taste, has no action on turnsole, and is not affected by solution of nitrate of silver.

The extremities of the gold wires, from whence the gas had proceeded, were oxidized, but that most perceptibly which communicated with the zinc pole of the pile. The whole gas obtained and collected during the experiment, was 793 cubic centimetres (1200 inches). The liquor remaining in the tube was carefully examined; it produced no sensation of taste on the tongue, had no action whatsoever on the tincture of turnsole and of fernambuc, and did not produce the least cloudiness in the solution of nitrate of silver.

The gas produced by the action of the pile was then examined. After having introduced one measure of it into the eudiometer of Fontana, an equal measure of nitrous gas, made purposely for the proof, was added; an absorption of seventy-

seventy-seven two hundredth parts took place in the volume of the two measures.

In order to ascertain whether all the oxygen which the gas contained had entered into combination in this absorption, a second measure of nitrous gas was introduced into the eudiometer; but the gas did not experience any diminution of its volume. The quantity of oxygen which the absorption produced by the introduction of the first measure of nitrous gas might indicate, was attempted to be valued by a comparison with atmospheric air essayed in the same manner; for which purpose one measure of atmospheric air was introduced, and an equal quantity of nitrous gas added; an absorption of fifty-five two hundredth parts took place in the volume of the two measures. In considering this absorption as the effect of the combination of nitrous gas with the volume of oxygen gas, corresponding to the 0,22 parts, which atmospheric air contains, it may be concluded, that the absorption of seventy-seven two hundredth parts, produced with the gas of the pile, represented proportionally the combination of the same quantity of nitrous gas with a little less than 0,31 parts of the oxygen. It was then observed that the measures of gas having been introduced separately and successively into the eudiometer, it might happen that they were not sufficiently well mixed, and that consequently the absorption was not as complete as it might be. It was thought that it might be better to pass the gases in separate measures under a jar, and then to introduce the whole volume together into the eudiometer. The former experiments having been repeated in this manner, an absorption took place between the gas of the pile and the nitrous gas, of ninety-two two hundredth parts in the volume of the two gases, in place of seventy-seven resulting from the same proof, by the first method; and with the atmospheric air and the nitrous gas the absorption was sixty-eight two hundredth parts instead of fifty-five. There results then from this, according to the same ratio of the 0,22 parts of oxygen contained in the atmospheric air, a proportional indication of about 0,30 parts of this gas contained in that of the pile. It was again proved with the eudiometer of Volta, by introducing a single measure into it, through which the electric spark was made to pass; the proof was afterwards repeated successively.

The gas produced is tried by Fontana's eudiometer, an absorption of seventy-seven two hundredth parts takes place.

The quantity of oxygen contained in the gas valued at 0,31 parts.

The proportion of the oxygen after more exact trials is more accurately valued at 0,30 of the gas.

The gas farther proved by Volta's eudiometer gives the same result of 0,30 oxygen.

successively, on two, three, and four measures, and always the absorption resulting from the inflammation by the electric spark, gave the same indication of about 0,30 parts of oxygen.

Experiment II.

Apparatus used in second experiment described. The sand is moistened with river water containing 1 fiftieth nitric acid. The gold wire is dissolved and precipitated,

Two grammes (31 gr.) of distilled water were put into a glass tube bent into the form of a syphon; two wires of gold of commerce of about 0,003 inch in diameter, passing into the water at about 0,024 inch distance from each other, were inclosed in this tube; the tube was then placed upon an horizontal pile of fifty double plates, of about $3\frac{1}{4}$ inches in each side. The intervals between them were filled up with dry sand, and then moistened with river water acidulated with about $\frac{1}{50}$ of nitric acid. The wires of gold having been placed in communication with the two poles of the pile, the water in the tube assumed in the first day a reddish brown tint at the side of the copper pole, and the wire which passed to that part was covered with a coat of oxide of gold of a deep brown colour. The wire which communicated with the zinc pole did not assume the same tint; the gold of the wire was dissolved by degrees, and was precipitated together with a part of the silver. This precipitate exhibited with a magnifying lens, over almost the whole length of the tube, crystals in needles. The wire corresponding to the zinc pole was entirely deprived of the gold which covered it, and then only consisted of a thread of silver of extreme tenuity. But very little gas was discharged from either extremities of the wires. The water was not diminished a fiftieth part of its volume.

very little gas produced, the water in the tube is not diminished 1-fiftieth.

The pile continued in action forty days. Indicates by the electrometer an intensity of 240° . The remaining liquor in the tube shews no trace of acidity by any of the reagents, and has a metallic taste.

The pile continued in activity 40 days from the 28th of Messidor to the 8th of Fructidor. It indicated then on the last day by the electrometer (simplified by one of the Members of the Galvanic Society, from that constructed in Germany, described in the *Journal de Physique* for the month Messidor, an. 13,) an intensity of 840 degrees. The liquor remaining did not afford, by any of the different reagents, the least trace of oxidizy; a metallic taste was alone perceptible in it.

The galvanic society, in examining chiefly the results of the first experiment, as corresponding more particularly with the fact announced by M. Pachiani, have considered, that in allowing

allowing for the small quantity of oxygen which had caused the oxidation of the extremities of the gold wires, the whole quantity of the oxygen contained by the gas of the pile may be valued in a very near approximation, at 0,31 of its volume; and it is very nearly in this same proportion that oxygen gas enters into the formation of water; it was thought that it might thence be concluded, that the whole effect of the galvanic pile, during the entire continuation of the experiment, had been the decomposition of a part of the water employed, and the separation, in their state of purity, of the oxygen and hydrogen gases which composed it. The Society then are of opinion, that M. Pachiani was deceived as to the nature of the acid which he announced that he obtained, and that this acid * might have been produced by some animal or vegetable substance employed in the apparatus. The Society does not hesitate to declare, that with the apparatus which they used in preference, (as being the most simple, and the least liable to the influence of other matters,) they do not think it possible to effect any thing by the action of the galvanic pile, but a decomposition more or less great, of the water used in the experiment.

The Society think the action of their pile only decomposed the water into oxygen and hydrogen gases, and that Pachiani is mistaken, and that the acid found in his experiment was produced by other means than those he announced, and that it is not possible to effect anything by the galvanic pile in water but its decomposition.

VIII.

Account of an Ancient Geographical Tablet in the Museum of Cardinal Borgia, from † a Memoir presented to the Academy of Gottingen, by PROFESSOR HEEVEN.

IN the Museum of Cardinal Borgia there is deposited an ancient geographical tablet, from which an engraving has been taken. The tablet was found in the museum of Cardinal Borgia, * M. Giobert, in Van Mon's Journal, pretends, that the acids and salts used in the pile circulate along the wires, and pass into any liquor into which they are conducted; which does not appear probable.—B.

The galvanic apparatus used in these experiments is called a pile through the whole of the French paper, though from its horizontal position the appellation does not seem very proper. *Trans.*

† This memoir is entitled, "explicatio planiglobii orbis terrarum faciem exhibens, ante medium sec. xv. summa arte confecti; agitantur simul de historia mapparum geographicarum recte instruenda consilia.

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M

been

The design is two feet in diameter, and done in coloured enamel,

limits of the countries not marked, accounts inserted in it of various remarkable things.

Why supposed to have been made in the first half of the fifteenth century.

It is the oldest geographical design, except the chart of Sanudo.

Sweden called Magna Gothia in it, Denmark omitted, Lithuanians called Pagans,

been made, one of the impressions of which is in the possession of the author of the Memoir. This remarkable monument is not a chart drawn by the pen, but a round tablet, of which the design occupies a space about two feet in diameter, on which the hemisphere, known at that time, is represented in coloured enamel, like a round surface. The countries and the places are marked by their proper names, but the limits of the countries are not traced; the mountains, the rivers, the people, and all the things remarkable (as the animals, the battles, the caravans, the bazars, the camps, the wandering tribes, &c.) are represented and explained on it by inscriptions in the latin tongue, but written in German characters. It may be conceived from this first view how interesting this monument is, and also with what art it is executed, so that it is impossible to suppose, that it was made for the use of a private person. Its date is not mentioned, but it may be determined with certainty that it was constructed in the first half of the fifteenth century. In reality, the most recent event marked on it is the victory of Tamerlane over Bajazet in 1402; there is no mention of the taking of Constantinople, or the least trace of any of the discoveries of the Portuguese. Of the geographical charts known at present, that of *Marino Sanudo* at the commencement of the fourteenth century is the only one certainly more ancient; but that of *Andre Bianco* of 1436, which *Formaleoni* has made known, is very nearly of the same time as this monument. No general source of information can be discovered by which the author of the tablet has been assisted. It is not made according to documents from Ptolemy; it more follows those of the Arabians, especially with regard to Africa; of the names which are found in the works of *Marco Paolo*, and the other more ancient travellers in Asia, only some are seen on that part of the world. The extent of Europe is represented as much greater than that of Africa, and at least as large as that of Asia. The following are some of the most remarkable particulars of it: Sweden is set down under the name of *Magna Gothia*, and Denmark is wanting. In Prussia, the seat of the wars of the Teutonic order with the Lithuanians, represented with this inscription: *Hic sunt confinia paganorum et christianorum, qui in Prusia adinvicem continuo bellant*. It may be perceived by this, that the Lithuanians were therein considered

sidered as Pagans, although christianity was introduced among them before this period. Russia appears under the denomination of Tartary, and near the Caspian Sea and the sea of Afos, are represented the famous Bazars of those times. England and Scotland appear at the border, but there is no more room for Ireland. Africa exhibits none of the discoveries of the Portuguese, but the northern half of it was known to the author as far as Soudan. He names not only the villages along the coast, but he moreover knows that the inhabitants of mount Atlas, the people of Barbary are at war with the Saracens. Near these mountains is inscribed, *In illis montanis habitant plures principes et reges, et habitant continuo in tentoriis, et praeliantur continuo contra Saracenos, et contra juxta castra et civitates* *. In Egypt the junction of the grand caravan of Mecca is marked, and not only the names of the deserts of sand are inserted, but those also of the places most important to commerce, as Tagaza, Ganusia, &c.

Russia called
Tartary in it.
Places of the
Bazars re-
presented.

It contains none
of the discoveries
of the Portuguese
in Africa.

The junction
of the Caravans
of Mecca noted
in Egypt.

The kingdom of Prester John extends in Nubia *ab ostio gandis* (Cape Gardesfan) *usque ad fluvium auri*. Bianco likewise sets down the kingdom of Prester John in Africa in the same manner, so that the Portuguese are not the first who have thus described it. Asia does not present fewer singularities. In Asia Minor the camps of the Tartars are represented; *Tartaria reges maxima, quæ Tartari cum suis jumentis et bobus excurrunt, civitatem ex multis tentoriis et carutes situant*. India is divided into *India superior*, where the body of St. Thomas is found, and many christian kingdoms, and *India interior, in qua Cathai civitas et magnificanis Imperatoris Tartarorum sedes*. China is likewise inserted in it, and its capital Cambalck (Cambalu Pekin.) is also named. On the frontiers of little Bucharra at Organli, (Urgang)

Extent of Prester
John's kingdom
set down.

Camps of the
Tartars in Asia
Minor marked.

The divisions of
India, and posi-
tion of Cathai
inserted,

and China with
its capital Cam-
balk (Pekin.)

* The Latin of the inscriptions in this paper is not very correct, *continuo* is used in them for *semper*, and *juxta* for *vicina*, the word *situant* is also improper, and some others, but these circumstances perhaps only mark more strongly the authenticity of the account. In the inscription relative to India interior, a small alteration has been made from the memoir in this translation. The word *magnificanis* has been formed from *magni canis* in the memoir, which being so printed, evidently was an error.—B.

The route of the caravans to Cathai; the country of Gog and Magog; and the site of Paradise.

de Organti ad Carthagium vadunt cameli in quatuor mensibus, the caravans going and returning to Cathai are represented. On the eastern border the country of Gog and Magog is set down, and finally locus deliciarum or paradise.

IX.

Analysis of Birdlime. By M. BUILLON LAGRANGE.*

SECT. I.

The Origin and Preparation of Birdlime.

Various opinions on the nature of birdlime;

THE substance known by the name and appellation of *birdlime*, has been classed among the immediate productions of vegetables.

M. Fourcroy,

M. Fourcroy was the first person who considered this matter to be glutinous: he has described it as a species in his "*Système des Connoissances Chimiques*," Vol. VIII. p. 306.

Birdlime, according to this chemist, may be made of the berries of mistletoe, or of the tender bark of the holly, and several other kinds of trees, macerated in water. Although this substance appears to have been hitherto not examined with sufficient accuracy, many qualities have been discovered in it analogous to those of gluten,

Excepting a few chemical properties, mentioned in my "*Manuel d'un Cours de Chimie*," third edition, I have never found in any work the least elucidation of the nature of this singular substance.

M. Chaptal,

M. Chaptal, in his "*Elémens de Chimie*," speaks only of its preparation. As the method prescribed by this chemist is nothing different from that in the "*Materia Medica*" of Geoffroy, and in the "*Dictionnaire*" of Valmont de Bomare, I shall quote the article itself.

Geoffroy, Valmont de Bomare.

Ancient mode of preparing it, by boiling and pounding the berries of mistletoe;

"The ancients made use of the berries of the mistletoe of oak in the preparation of birdlime. The berries being first boiled in water, were pounded, and the hot water was then poured off, in order to carry away the seeds and rhind. At present birdlime made of the bark of holly is preferred. The middle bark is made choice of, as being the most tender and

* *Annales de Chimie*, Vol. LVI.

green: this is placed in a pit to rot, after which it is pounded in mortars until it becomes a paste, and is then washed and cleansed with water. This substance has been considered as discutient and emollient, when applied outwardly."

at present it is made from holly bark rotted and pounded.

It is already known that the mistletoe of oak is employed in several pharmaceutical preparations; as, the universal water, the antispasmodic powder, Guttet's powder, &c.

Mistletoe of oak used in pharmacy.

In England, according to Geoffroy, birdlime is made of the bark of holly. He says, the bark is boiled in water seven or eight hours, till it becomes soft and tender. This is laid in masses in the earth, and covered with stones, placing one layer over another—the water having been previously drained from the bark. In this state it is left to ferment and rot, during a fortnight or three weeks, in which time it changes to a kind of mucilage. It is then taken from the pit, pounded in mortars till reduced to a paste, washed in river water, and kneaded till freed from all extraneous matters. The paste is left in earthen vessels during four or five days, to ferment and purify itself. It is afterwards put into proper vessels, and thus becomes an article of commerce.

English mode of making birdlime:

Bark of holly is boiled, rotted, pounded, washed, and kneaded.

This mode of preparation is not universally followed, as every county has its peculiar way: there are even those who make a secret of the process.

At Nogent-le-Rotrou birdlime is manufactured by cutting in small pieces the second bark of the holly, fermenting them in a cool place during a fortnight, and then boiling them in water, which is afterwards evaporated.

Method pursued at Nogent-le-Rotrou; first rotting and then boiling:

At Commerci and its environs birdlime is obtained from several shrubs, as the holly, the wild vine (*viburnum lantana*, Lin.) and the mistletoe of every species.

At Commerci.

The best is that made from the prickly holly, which is greenish; that obtained from the *viburnum lantana* is of a yellowish tint. In using this latter, the epidermis is rejected, and only the second bark employed.

Best sort from the prickly holly.

The birdlime which I used in my experiments was made from the second bark of holly; and on comparing it carefully with some which had been sent me from Commerci, I found there was no apparent difference between them. I thought this precaution essential to obtaining greater precision in my analysis. It is well known that the birdlime of commerce is never in a pure state; it is frequently a composition of vegetable

table

Birdlime of commerce frequently adulterated.

table and animal matter; sometimes it is even adulterated with turpentine, oil, vinegar, &c. It was therefore necessary that I should be certain as to its purity; and by the following mode, which I adopted, I obtained birdlime of the very best quality.

The author's preparation of birdlime.

Having procured a sufficient quantity of the second bark of holly, I bruised it well, and boiled it in water for four or five hours: the water being poured off, I deposited the bark in pits, in earthen pans, where it remained till rotten, or at least till it became viscous, moistening it from time to time with a little water. When it had obtained a proper degree of fermentation, it was cleansed, by washing, from all heterogeneous matters.

SECT. II.

Chemical and physical Characteristics of Birdlime.

Characteristics.

Birdlime is of a greenish colour, and of a sour flavour: it is gluey, stringy, and tenacious. Its smell resembles that of linseed oil.

It becomes dry and brittle by exposure to the air;

Spread on a glass plate, and exposed for some time to the action of air and light, it dries, and becomes brown in colour, being no longer viscous. When quite dry, it may be reduced to powder, in which state it is totally divested of its adhesive qualities, and only recovers them on the addition of water.

but viscid again when wetted.

Birdlime reddens tincture of turnsole.

By moderate heat it fuses.

When gently heated in a porcelain vessel it melts, but does not become very liquid; it swells in bubbles, which float upon the surface. This kind of fusion produces small black grains, which render the birdlime grumous: it produces a smell very similar to that obtained from animal oils, on raising their temperature.

If this fusion be continued for some time, the birdlime assumes a brownish colour; but recovers its proper characteristics on cooling.

Strong heat inflames it;

Placed on red-hot coals, it burns with a brisk flame, and creates a great deal of smoke.

Heated in a crucible of platina, it takes fire when the crucible is red-hot; produces a lively flame, which rises to about two decimetres, accompanied with a considerable quantity of smoke,

smoke, which easily attaches to the chimney: this combustion always takes place, although the crucible be taken out of the fire.

A whitish residue is left, which is very alkaline and partly soluble in water. Re-agents demonstrate the presence of sulphate and muriate of potash. leaving an alkaline residue.

That part which water could not take up, on being put into muriatic acid was dissolved with some effervescence. Experiments on the residue.

This liquid is copiously precipitated by the oxalate of ammonia; prussiate of potash gives a blue precipitate. That produced by ammonia is of a pasty consistence, partly soluble in caustic potash: whence may be inferred that the residue, independent of the salts soluble by water, is composed of the carbonates of lime and of alumine, with a small portion of iron. It contains soluble salt and carbonate of lime, and alumine and iron.

Water has very little influence on birdlime. On boiling, the matter does not completely dissolve, but acquires merely a small increase of fluidity, which it loses in cooling, and resumes its primitive consistency. Water has little action on birdlime.

This water obtains no colour; its flavour is at first insipid, afterwards sour, and it reddens tincture of turnsole.

Evaporated to the consistency of a syrup, it becomes coloured, with a mucilaginous appearance, which may be separated by alcohol.

The action of water, therefore, is confined to the solution of a mucilaginous substance, with a small portion of the extractive matter. It takes up some mucilage.

It is not thus with caustic potash. Its concentrated solution forms at once with birdlime a whitish magma, which turns brown on evaporation, with a separation of ammonia. Caustic potash combines with birdlime and gives out ammonia.

This composition is less viscid; it acquires a great degree of hardness by exposure to the air; and its smell and taste are similar to those of soap.

It is chiefly soluble in water and alcohol, there remaining but a few vegetable dregs. These solutions are affected by strong acids; but these kinds of decompositions present no new phenomena to those obtained with a solution of soap. The compound has the habitudes of soap.

The most feeble acids soften birdlime, and partly dissolve it: when concentrated, they act in a different manner. Weak acids soften and partly dissolve birdlime.

Sulphuric acid renders it black and charry: by adding powdered lime, so as to form a thick magma, a separation of ac- Sulphuric acid chars it.

tic acid and ammonia is procured. There can be no doubt but that in addition to the acetic acid naturally present in birdlime, more is produced by the action of the sulphuric acid.

Hot nitric acid decomposes it, and forms a kind of wax:

Nitric acid, whilst cold, has very little power over birdlime: but on increasing its temperature, the mixture turns yellow, dissolves, and as evaporation advances swells considerably, leaving at last a hard brittle mass. This mass, when a second time submitted to the action of nitric acid, is dissolved, a part being converted into malic and oxalic acids. By continuing the evaporation a yellow matter is obtained, easily friable, yielding to the pressure of the fingers like wax, with a kind of elasticity, and melting by means of a gentle heat.

which forms soap with potash.

Alcohol partially dissolves it.

Potash combines with this matter, changes its colour from yellow to brown, and forms perfect soap.

Alcohol partially dissolves it, and becomes yellowish; its transparency is diminished by the addition of water.

On evaporating the alcohol to dryness, there remains a yellow matter divested of the greasy appearance, which yields a sweet odour in burning.

Cold muriatic acid has little action on birdlime.

Oxygenated muriatic acid alters birdlime considerably.

Cold muriatic acid has no action upon birdlime: when heated it turns it black.

Oxygenated muriatic acid operates differently. Either by mixing the gas with the water containing the liquid birdlime, or by shaking it in a bottle with the acid in a very concentrated state, the following phenomena were equally observed:

The birdlime quickly lost its colour, and became white; it was no longer viscid, but divided into hard compact portions, containing in their centre a quantity of birdlime, which the oxygenating principle had not reached. This non-oxygenation may be attributed to the difficulty there is of preserving this substance in its liquefied state in hot water, whereby the operation of the acid is confined to its exterior surface.

Characteristics of oxygenated birdlime.

The characteristics of oxygenated birdlime are—

1. It is capable of being reduced to powder.
2. It is insoluble in water, even when heated.
3. It does not become liquefied at a high temperature.
4. It will not turn yellow, nor will it form a resin with nitric acid.

Acetous

Acetous acid softens birdlime and dissolves a certain quantity; the liquor acquires a yellow colour; its taste is insipid.

Acetous acid softens birdlime, &c.

Carbonate of potash produces no precipitate; evaporation gives a resinous residuum, which cannot, however, be brought to a state of perfect dryness.

Certain metallic oxides are easily reduced on being heated with birdlime.

Birdlime reduces metallic oxides.

Semi-vitreous oxide of lead assumes a grey colour, dissolves, and forms with the birdlime an emplastie mass.

Semi vitreous oxide of lead incorporates with birdlime.

Alcohol at 40 degrees, and boiling, dissolves birdlime so long as it is kept hot; it is clear, and of a transparent yellow colour; but in proportion as the liquor cools, it becomes turbid.

Boiling alcohol dissolves birdlime,

A yellow matter may be separated by filtering, which is much softer than the original mass; melts in a moderate heat, diffusing a smell analogous to that of wax, of which it seems to possess all the qualities.

and lets fall a kind of wax by cooling;

The filtered liquor is bitter, nauseous, and acid; precipitating in water, and leaving on evaporation a substance similar to resin.

retaining a resin.

Sulphuric ether may be considered as the true solvent of birdlime: its action on this substance is rapid, first dividing it, and then dissolving it nearly *in toto*, there remaining only a few vegetable dregs. The liquor acquires a greenish yellow colour, and strongly reddens turnsole. On adding a little water the mixture thickens, and the ether swims on the top; but if a sufficient quantity of water be poured in to dissolve the ether, a body of oil is formed on the surface, bearing a considerable analogy to that of lintseed: this may be converted, by the assistance of the semi-vitreous oxide of lead, into an emplastie mass.

Sulphuric ether the true solvent of birdlime.

Water separates oil from the solution.

By evaporating the solution of birdlime in ether, a greasy substance is obtained, of a yellow colour, and of the softness of wax.

Ether by evaporation gives a greasy substance.

Conclusion.

From the foregoing observations it will be perceived how little analogy exists between birdlime and gluten.

A simple comparison will be sufficient to designate the place it ought to occupy among vegetable productions.

General properties of birdlime.

Birdlime

Birdlime is viscid, elastic; dries a little in the air, by exposure to which it becomes brown; but is not rendered brittle and irrecoverable like gluten.

It melts in the fire, swells, and burns with a vivid blaze; but does not diffuse that animal odour which is to be observed in gluten.

Water will not dissolve birdlime; it merely imbibes the mucilage, the extractive matter, and the acetic acid.

Alcalies dissolve it; when concentrated, they convert it into soap.

Dilute acids soften and partially dissolve birdlime.

Concentrated sulphuric acid renders it black and charry.

Nitric acid turns its colour to yellow, converting the substance partly to malic and oxalic acids, and partly to resin and wax.

Oxygenated muriatic acid renders it white and solid, constituting *oxygenated birdlime*.

Alcohol exerts but little action upon birdlime; it dissolves the resin and destroys the acid.

Lastly, sulphuric ether dissolves it entirely.

Birdlime, therefore, differs from gluten,

1st. In the acetous acid which exists in it.

2d. In being very slightly animalized.

3d. In the mucilage and extractive matter which may be obtained from it.

4th. In the great quantity of resin which may be obtained from it by means of nitric acid.

5th. In its solubility in ether.

Recapitulation
of the points
wherein it dif-
fers from gluten.

X.

*Method of purifying Oil. By M. CURAUDEAU.**

The purification
of oil consists in
its clarification.

THE purification of oils for combustion consists solely in their clarification: It is only since Argand's lamps have come into common use that this subject has received much attention.

There are many processes for the purification of oils, but all are not equally good; and those who sell purified oil make a secret of the method of purification.

* From Cours complet d'Agriculture, Tome XII.

However,

However, as the art of purifying oils ought to be known by those who manufacture them, the processes, which are considered the most economical and simple, shall be here mentioned; by which information they will be able to obtain that profit which those now make who follow this species of industry after them.

The process for the purification of oil by sulphuric acid, which follows, is little different from that published by Thenard. Process by sulphuric acid described.

To one hundred parts of rape oil one part of sulphuric acid is to be added, diluted with six times its weight of water; the mixture should be strongly agitated, and as soon as this is completely finished, it is left still till the oil becomes clear; when it is perfectly clear the purification is effected. Sulphuric acid and water added to the oil; the mixture strongly agitated, and left to settle.

There remains at the bottom of the vessel an acid liquor somewhat coloured: the oil is to be separated from the sediment; and in order to be certain that no acid is retained by the oil, some ounces of powdered chalk is to be added; the mixture should then be shaken, and the oil again left quiet to settle. The oil separated from the sediment; powdered chalk added; agitated again, and left to settle.

The action of the sulphuric acid in this process consists in depriving the oil of all its humidity, although it is itself mixed with water, and in separating from it a mucofo-extractive substance, the presence of which diminishes the energy of the combustion of the oil, covers the wick with charcoal, and produces much smoke: It is then on the abstraction of these principles foreign to the oil, that its quality of giving a good light depends. The acid in this process separates the mucofo-extractive matter from the oil, which injures its combustion.

Another Method.

The next process to be described has been followed by some manufacturers, who have had good success with it. Process by flour and water.

To one hundred parts of rape-seed oil ten parts of water are to be put, to which one part of wheaten flour has been added; the mixture is to be well agitated, and then to be heated until all the water added has been evaporated, or, more properly, until the oil has ceased to have any union with the substances which it held in suspension: In this state it becomes purified; and at the end of twenty-four hours it is very clear, and does not differ at all in quality from that prepared by sulphuric acid. Flour and water added to the oil; the whole well agitated, and heated till a separation of the contained matters ensues; in 24 hours after this it is clear.

In

The heat should be applied gradually, and should not exceed 80° Reaumur.

In the practice of this last process, care should be taken to heat the oil gradually, and not to raise its temperature above 80 degrees of Reaumur's thermometer. (212 Fahrenheit) This heat is sufficient to effect the coction of the flour, and of the mucoso-extractive matter contained by the oil; a greater degree of heat would colour the oil, and deprive it of the appearance most favourable to its sale.

M. Curaudeau led to this process by observing the separation of white sauce into two substances when too much done.

M. Curaudeau was led to this process by an observation, which every one may likewise make. It is well known that the sauce called melted butter, when too much boiled is separated into two parts, one which is thick and occupies the bottom of the vessel, while the other part is clear and floats above the first: The lower substance is the caleous part of the butter united to the flour that has been added to the sauce, and which the action of the fire has separated from the oil; The upper substance is the butter deprived of all foreign matter; and in this state it may be called *purified butter*.

XI.

*On a peculiar Fluctuation of the River Dordogne, called the Mascaret. By M. LAGRAVE SORBIE *.*

The mascaret takes place only when the waters are low.

THE peculiar movement of the waters of the River Dordogne, which is called the *Mascaret*, takes place twice each day in the summer time, when the waters are low, which is an essential condition. A similar motion also takes place on the river Amazons according to the report of M. de la Condamine, when it is named the *Pororoca*; the same is also perceived at the Orcades, off the north of Scotland, according to this author: and M. Sorbie has seen accounts in the publications of some voyagers of its likewise occurring in some of the rivers of Hudson's bay, and also in the Mississippi.

The Pororoca of the Amazons similar to it, a like occurrence at the Orcades, and in the rivers of Hudson's bay, and in the Mississippi.

It is not surprising that this phenomenon does not happen in all rivers; it is not always seen even in the Dordogne. From the most exact observation, if the summer is not dry, and that the waters are not low to a certain degree the *Mascaret* does not appear, it rarely occurs in winter; it however

sometimes takes place during very hard frosts, when the cold has diminished the waters by the formation of much ice; but this happens very seldom, and has never been more than three times, in several ages.

There is a *maximum* of depression in the waters necessary to its appearance; Wherefore the mariners in the neighbourhood of Bourdeaux are in the habit of talking of it somewhat in this manner, "The waters have fallen so much, the tide will encrease to day to such a height, we shall have a Mafcaret", and they load their barks accordingly, and take precautions to avoid it. The manœuvres of these mariners have caused some naturalists in the vicinity of Bourdeaux to observe long since, that this phenomenon must depend on a natural cause, connected with the bed of the river, since these men can foretel, without being scarcely ever mistaken, by the depression of the water, whether the Mafcaret shall appear or not, although sometimes it has not appeared before for some years, because the rains have prevented the waters from diminishing to the necessary degree.

No one has yet tried to explain the cause of this singular fact, not even M. Condamine, or if there be any works on the subject, they are unknown to the author though his studies have been particularly directed in the line where such information might occur, and he has read much. In order to enable others to account for the facts, he mentions those which relate to it such as he has himself seen, and such as he has been told have been witnessed for several ages.

In the summer, or, more properly speaking, when the waters are low, there appears at a little distance from the junction of the Dordogne with the Garonne, or at Bec d'Ambes, an accumulation of water, like a promontory, on the shore, which is from the thickness of a ton to that of a small house, and which rolls along with such velocity that no horse, whatever might be his speed, could keep up with it. It follows the direction of the shore, and makes a most frightful noise. The horses and oxen, which feed in the meadows near the river, run away with their utmost speed exhibiting the greatest terror; so much so that they remain trembling a long time after, and cannot be driven back but with much difficulty. The ducks and geese have also been seen to precipitate themselves into the reeds at its approach, with the greatest speed and as-
fright.

The mariners foretel the Mafcaret from the lowness of the river,

no account of the cause of the Mafcaret yet published.

It consists of an accumulation of water, which appears first at Bec d'Ambes, and rushes up the river with great velocity, and a frightful noise, terrifies the cattle, and the water fowl,

overturns the
 piers along the
 river, and drives
 the large stones
 which compose
 them, more
 than fifty paces
 off,
 tears up large
 trees by the
 roots, and sinks
 and breaks ves-
 sels in pieces.
 Above St. Andre
 it appears in
 waves, above
 Asque is seen in
 its original form;
 in waves again
 above Lile, at
 Tersac it re-
 gains its first
 appearance;
 at Fronfac it
 occupies the
 whole breadth
 of the river,
 passes before
 Libourne with
 a terrible noise,
 and ceases at
 Peyrefite.
 Account of the
 Pororoca on the
 Amazons:

its noise heard
 at a league
 distance.

It advances in
 several waves,
 each twelve or
 fifteen feet high,

fright, and lie flat there, without being able to come out. Hard bodies, which lie in the way of the Mascaret are struck by it with such force, that the piers, built for the use of the vessels, along the shore are demolished, and some of the stones which compose them, although very large, are driven away more than fifty paces; the strongest trees are torn up by the roots, the barks which it meets are not only sunk, but broken, asunder, especially if they are near the shore, or have any hard body lying beneath them. From the place called St. Andre (See the lower part of Plate IV.) on the river, the Mascaret forms itself into waves which half its breadth as far up as Caverne; there it disappears for a short time, to appear again between Asque and Lile like a promontory, and then returns into the form of waves as far on as Tersac; at Tersac it regains its first appearance, which it only quits at Vayne; from Vayne it proceeds along the bank as far as Fronfac, the house of M. de Richelieu; from Fronfac it occupies the whole breadth of the river, passes with a terrifying noise before the village of Libourne, throws the road for vessels belonging to this village into confusion, and afterwards appears at Genisac-les Reaux and at Peyrefite with but very little force. The whole passes in the course of seven or eight leagues.

The following is the account, which M. la Condamine gives of the *Pororoca* of the river Amazons, the comparison of the effects of which with those of the Mascaret will tend to establish the theory of these phenomena.

In his voyage to the river Amazons, page 193, he relates, that "between Macapa and Cape-Port, where the channel of the river is most confined by the islands, and especially opposite the mouth of the Arawary, which joins the Amazons on the north side, the flowing of the sea exhibits a singular phenomenon. During the three days next the full of the new moons, the times of the highest tides, the sea, instead of taking almost six hours to rise arrives at its greatest height in one or two minutes; it may be conceived that this does not happen quietly; there is heard at a league distance a terrible noise, which announces the *Pororoca*, which is the name that the Indians of these parts give to this frightful flood. In proportion as it approaches the noise encreases, and soon an accumulation of water, like a promontory, appears from 12 to 15 feet high; after that another is seen, then a third, and sometimes a fourth, which follow each

each other closely, and which occupy the whole breadth of the channel. These waves advance with a prodigious rapidity, which rush forward with great rapidity, break and overturn every thing which opposes them. I have seen in some places a large extent of land carried away, great trees torn up by the roots, and ravages of all kinds committed; every thing which opposes them, carry away large portions of the land, and tear up trees by the roots. every where that they pass the banks are swept clean; the canoes, the pirogues, and even the barks can only escape their fury, by anchoring in deep water. After having examined this phenomenon with attention in different places, I have always remarked that it only takes place, when the rising flood is engaged in a narrow channel, or meets in its way with a bank of sand, or a shallow place, which occasions an obstacle to it; that it was in those places alone that this impetuous and irregular movement of the waters commenced, and that it ceased a little beyond the bank, when the channel became deeper, or grew considerably wider. It is said that something similar to this happens at the isles of the Orcades, at the north of Scotland, and at the entrance of the Garonne, (it should be the Dordogne), in the vicinity of Bourdeaux, where the effects of these tides, is called a *Mascaret*." It only occurs in narrow channels, over sand banks, or shallow places.

It appears from what has been cited from M. Condamine, that the effects of the Pororoca are almost the same as those of the Mascaret. Nevertheless there is a marked difference between them in this respect, that on the Dordogne, two kinds of floods take place, one which extends over the whole river, and is similar to that which M. Condamine has observed, and the other which ranges along the shore, rolling more over the deposits which the waters have left, than in the water itself. He says positively in page 194, that "at one or two leagues a frightful noise is heard, which announces the Pororoca; as it approaches the noise encreases; and soon an accumulation of water appears from 12 to 15 feet high, and then another that follows, which occupies the whole breadth of the channel". On the Dordogne the Mascaret rises with great noise, sometimes along the coast in an elongated accumulation, and sometimes in the form of frightful waves, which extend over the whole river; when it follows the shore it only appears in the re-entring angles, and on the sand banks, as is described in the sketch of the plan of the river, which accompanies this account, and which takes in the whole extent where the effects of the Mascaret are perceived. The parts covered with small Description of the sketch of the course of the

Mascaret in the small points, indicate the sand banks where the Mascaret always commences; the parts occupied by small lines, are the places where the waves occupy the whole breadth of the river. The dotted parts indicate the re-entring angles, where the sand banks are found which are deposited by the counter current.

It is here, principally that the Mascaret rolls with all its fury over the mud of the river. On the banks the salient angles, are the places where the Mascaret quits the shore, occupies the whole river, and runs upwards, accompanied by many considerable waves, which succeed each other, till another re-entring angle occurs, where it again resumes its first form.

It is thus that these who dwell in the vicinity of Bourdeaux witness without emotion twice each day, when the waters are low, so extraordinary a phenomenon, without any one thinking of examining into the cause of it, or even of communicating the particulars to naturalists.

The tide is the primary cause of the Mascaret.

The primary cause of this rising of the water is the same as that of the tide in all rivers; and if the Mascaret occurs on very few rivers, it is because their beds are not formed in a manner necessary to produce it, and have not the same disposition as the Gironde and Dordogne: they have either too little or too great a current; their waters are not sufficiently low, or when they are, the tide does not continue long enough; finally the re-entring and salient angles are not such as they ought to be. M. Sorbie thinks he could tell before hand whether any river would be liable to such effects, from the form of its plan and the disposition of its bottom; and is of opinion that the cause why more rivers are not subject to the Mascaret, depends entirely on the shape of their beds, and not on any particularity in their tides. The physical cause of that on the Dordogne appears very simple, M. de la Condamine says, that on the Amazons it is always at the narrow parts where it is observed. The cause is not the same on the Dordogne, for there is no narrow parts in almost its whole course: it is nearly every where very rapid, and of small depth, as all those rivers are which have much current. It forms, as may be seen in the plan, many turns and windings; and has few isles: but at each angle a bank of sand is deposited: It descends, notwithstanding these windings, almost from the east to the north-west. As far as Bec d'Ambes, where it unites with the Garonne which is much more powerful than it, and they form together that beautiful arm of the sea, called the

The course of the Dordogne described, to account for the Mascaret,

the Gironde. The two rivers then descend together from Bec d'Ambes to the sea in a direction from the east to the north-west. All the waters which arrive from the arm of the sea or from the river, advance in a straight line with abundance into the mouth of the Dordogne, instead of mounting up the Garonne, which runs almost north and south as far up as Bourdeaux. The greatest part of the waters which are advancing to the Garonne, ought then, when the current has taken its course, to run up the Dordogne at the beginning of the flood, since its velocity does not allow them time to turn up the Garonne; and thus the water which ought to go to the Garonne, running up the Dordogne, form by their abundance, this effect which Condamine recites: He says that "the tides, which usually take six hours to rise, arrive at their full height in one or two minutes". But on the Dordogne, the tides never come to their highest level in near so short a time, even when the waters are lowest; but in one or two minutes they encrease considerably; which encrease is probably caused by the waves which arrive almost instantly; and the flood raising their masses of water above their natural level, leaves them there to augment the water in the bed of the river in proportion to their bulk. After the Mascaret has passed, the waters of both rivers encrease in the same gradual manner as those of all other rivers.

M. Sorbie likewise thinks, after all, that the tide of the Gironde may be the cause of the Mascaret on the Dordogne, for it pours its waters into the mouth of the Dordogne in almost a right line; this arm of the sea being at least six times larger and deeper than the Dordogne, ought at the flood to carry up such an abundance of water, as could not enter into the bed of this river without occasioning the accumulation of waters described. The physical cause then of the Mascaret is the considerable mass of water which arrives from the Gironde into the mouth of the Dordogne, and the small depth of this river; since it is known that in rainy seasons, and when the river is a little encreased in size, this circumstance never takes place.

M. Sorbie remarks in conclusion that the facts related shew evidently that the flowing and ebbing of the tides of rivers are different from those of the sea; that the ebbing and flowing of rivers, are only secondary effects of the tides of the sea; that is to say, that the waters of the sea only form a dam to those

supposed to be caused by the waters on their way to the Garonne, taking the straighter course up the Dordogne.

It may also be caused by the tide of the Gironde rushing in a right line into the mouth of the Dordogne, and by the shallowness of this river,

those of the rivers, and that the rivers form by the abundance of their waters, those rapid flood-tides which are observed on the great rivers, such as those of the river Amazons, which ascend from 5 to 100 leagues, those of the Senegal, which advance almost as far up, and those of other rivers almost equally considerable. M. Sorbie thinks that the Mascaret, or the Pororoca, have altogether the same cause as the flood-tide of rivers, and though some slight secondary effects occur, such as those related, that all arise from the same physical cause.

XII.

Description of a secret Lock of ten thousand Combinations.

W. N.

Disquisition
upon locks.
The common
lock.

Bolt, key,
wards, pick-
locks, skeleton-
keys.

THE common lock usually consists of a bolt, which requires a particular instrument, called the key, to push it backward and forward; and in order that this bolt may be inaccessible to violation, certain impediments or obstacles, usually called wards, are interposed between the key-hole and the bolt, which make it difficult to open the lock by any general or common process. The general process for picking a lock, of which the key has not been seen, consists in operating upon the bolt by a small bended instrument or wire; or else by endeavouring to discover the position of the wards by an unperforated key, on the face of which some soft or plastic matter is lodged. And when this situation is once discovered, it is not difficult to file away so much of the key as shall allow it to pass, or else to select, out of a number of skeleton keys, one, of which the form shall admit of its passing through the lock. There are many locks so situated, as for example in the vestries of churches and other little frequented places, as to admit of this slow operation; but it must at the same time be allowed, that the English market presents locks of a number of different constructions, which can neither be picked nor analyzed by the process here mentioned. Nothing is more common, however, than for keys to be entrusted out of the hands of the possessor, or to be hung up, or casually laid down or mislaid. In these circumstances their figure may be taken with

with wax, like the impression of a seal, or more speedily by indentation upon a piece of moistened paper, or by various other means; and it must be admitted, that very little skill is required to enlarge the openings of a common key, so as to make it pass the wards of a superior lock.

These necessary and unavoidable imperfections of common locks, have long ago led to the introduction of secret locks, which are so constructed as to require some particular manipulation in opening them; such as that the key should be turned twice round, or that it should be turned through a certain space in one direction, and then back again; or that it should act upon some delicately resisting piece, very likely to be disregarded by an uninstructed possessor of the key; or that a number of visible parts should be placed in some determined order, before the common process of opening, either with or without a key, can take place. Upon all these contrivances one general remark may be made, namely, that the possessor must always in person open his own lock; for if this be to be done by the mere practice of a secret without a key, his cabinet becomes for ever open to him who, by communication or otherwise, shall possess that secret; and if a key be used, his lock, as to that person, becomes as subject to violation as a common lock.

In the mechanical consideration of a secret lock, we may suppose the construction to be entirely unknown to him who is desirous of opening it. In this, according to the experience and sagacity of the operator, the difficulties will be greater or less, and a very shallow contrivance may occasionally present a greater obstacle than a much more elaborate structure. But if we suppose the system of the lock to be known, but the particular conditions of opening it to be secret, the examiner will then take for his guide the probable circumstance that the re-action of the parts may feel considerably different, when they are duly placed for opening, than when their situation is such as to prevent that effect. By this clue, and by careful examination, most of these locks may be opened; and it is remarkable, that the better the workmanship the more easy it is in general to make the intended discovery.

The following are the conditions which appear to me to be necessary in a lock of the most perfect kind:

N 2

1. That

Secret locks;
their structure
and imperfec-
tion.

Methods of vio-
lating them.

Conditions of a
perfect lock
enumerated.

1. That certain parts of the lock should be variable in position through a great number of combinations, one only of which shall allow the lock to be opened or shut.

2. That this last mentioned combination should be variable at the pleasure of the possessor.

3. That it shall not be possible, after the lock is closed and the combination disturbed, for any one, not even the maker of the lock, to discover by any examination what may be the proper situations of the parts required to open the lock.

4. That trials of this nature shall not be capable of injuring the work.

5. That it shall require no key;

6. And be as easily opened in the dark as in the light.

These conditions are in some respects liable to the inconveniences already mentioned. I would therefore add the following conditions:

7. That the opening and shutting should be done by a process as simple as that of a common lock.

8. That it should open without a key, or with one, at pleasure.

9. That the key-hole be concealed, defended, or inaccessible.

10. That the key may be used by a stranger without his knowing or being able to discover the adopted combination.

11. That the key be capable of adjustment to all the variations of the lock, and yet be simple.

12. That the lock should not be liable to be taken off and examined, whether the receptacle be open or shut, except by one who knows the adopted combination.

Description of a
new lock of
combination.

In meditating upon this mechanical problem, I have thought of various constructions, but have not yet matured one in which all the above conditions are complied with. The lock delineated in *Plate III.* possesses the first six requisites. *Fig. 1.* represents the plate of the lock, of which the other side is seen at *Fig. 4.* In this last figure the middle piece is a handle or knob, represented *Fig. 6,* which, when turned, serves to shoot the double bolt *k*, *Fig. 1,* by any common connection. In the actual lock this bolt is carried backward and forward by a pin standing out of *Fig. 2,* soon to be described. The other four circles in *Fig. 4,* are handles, represented in *Fig. 5,* which serve

serve to move the four wheels seen in *Fig. 1*. These wheels have twelve teeth each, and are fastened by center-screws, each upon a flat wheel of the same tooth; but having only ten notches actually cut, as is seen in the right hand upper corner, where one of the upper wheels is taken off, and is shewn at *Fig. 3*. These upper wheels have their toothed part considerably higher than the interior or flat part; so that they would be contrate wheels if the teeth were cut quite through. But this is not the case, except with two of the notches, as may be seen in the two lower wheels more particularly, and also in the others. The upper wheels have also two of the notches between the teeth stopped up, as is shewn in *Fig. 3*; by which contrivance there are but ten situations for screwing each wheel upon its correspondent under wheel; and these situations are rendered precise, and all relative motion between the two correspondent wheels prevented by a small stud seen in the uncovered wheel, *Fig. 1*, which fits into one of the notches of the upper wheel when put in its place. The upper wheel has a number on each tooth from 1 to 9 and 0, which are of use for placing this stud. The four under wheels are held in their situations by four spring-catches, which allow them to be turned, in one direction only, by means of their knobs or handles; and when any wheel is thus turned round, the finger and thumb will feel the stroke of the lever, as it successively falls into each notch, until the lever comes to rest upon the smooth part. This very palpable indication then shews when to begin to count, calling the first hold or stroke of the catch 1; the second 2; the third 3, &c.; and the lock is so constructed, that when the top wheel of any of the four couple is put on with any number opposite the stud, the same number counted by the catch will place the upper wheel in such a situation, as that its notches, which pass clear through, will lie in a circle described from the center or axis upon which the great handle turns. And therefore, when each of these wheels is put in its place, and the numbers known (and registered, or put in the memory by some artificial association, such as of the date of the year taken either backwards or forwards, &c.) it is only needful to move each of the four knobs till its catch has passed the smooth part, with a number of strokes answering to its adjustment, and the circle indicated by broken shaded lines in *Fig. 1*, will be capable of passing through

Description of a
new lock of
combination.

Description of a new lock of combination.

through the open spaces of every one of the wheels: *Fig. 2.* represents a contrate wheel, having its irregular portions A, B, C, D, &c. standing up above its plane. These portions are parts of a circle equal to that denoted by the broken shaded parts in *Fig. 1.* The contrate wheel is to be placed in *Fig. 1.* with its face turned down; and being there secured with its center to the central handle, it serves to open and shut the bolt, which it can only do when the four wheels are in such a situation as to allow the circular edge-parts of *Fig. 2.* to pass clear through their notches. If any one or more of those wheels be turned so as not to correspond with its number, it will be impossible to turn the handle, because every attempt to do so will cause one of the parts of *Fig. 2.* to stop in one of the notches of the wheels through which it cannot pass. The method of opening the lock will therefore consist in setting each wheel to its known number.

As the proper situation of each wheel is only one out of ten, it is nine to one against any operator upon this lock, that he shall not set the first wheel right, supposing all the others in their due positions; but it is true that he may try all round, and will come to the right place at last. If two only of the wheels were deranged, it would be eighty-one to one that he should not set them both right; and he would be deprived of any trial round a single wheel, because the other wheel would always hold against him, and prevent his knowing when the open notch of the wheel under trial presented itself. Three wheels deranged would make the odds 729 to one, and the four would make the odds 6561. In the plate the combinations are said to be ten thousand, from an oversight in taking the ratio of ten to one instead of nine to one. But this is a matter of no consequence as to the principles of the lock, because the number of teeth or number of wheels are capable of variation. If a fifth wheel were added to this lock, the odds would amount to 59049.

As the quantity cut from *Fig. 2.* is not more than was necessary for the clear rotation of the wheels when the lock is shut, this piece, when in every other position, prevents the other wheels from being turned at all.

XIII.

*Letter from Mr. ALEX. CROMBIE, concerning the Caledonian
Literary Society at Aberdeen.*

To Mr. NICHOLSON.

SIR,

THE want of societies for scientific and literary improvement, has been long felt in many considerable towns in Scotland, and I believe in none more than in Aberdeen.

The utility of such institutions being so generally acknowledged, it is truly a matter of surprize to find so few of them in this kingdom, especially when the facility of forming them is considered. Any attempt, however small, to promote the interests of literature, and to diffuse moral, political, or philosophical knowledge among men of all ranks, will ever meet with the marked approbation of the sincere wellwisher to his country; and I am persuaded you will receive peculiar satisfaction in being able to communicate to the public the feeblest efforts which may be at any time directed to so important and desirable an object.

Great utility of societies for scientific and literary improvement.

In your Journal for December last, a traveller has expressed his surprize to find no antiquarian or literary society, or subscription library, at Aberdeen; and I agree with his remark, that those who know the respectability of the place, cannot fail to be astonished at it. To account for so singular a fact would perhaps be deemed presumptuous. I have too much respect for my fellow-citizens to attribute it to a want of taste, but I cannot help blaming those amongst us who are qualified for supporting such institutions, for their want of attention in this respect.

Reference to a letter in a former Journal.

The Professors of both Universities certainly unite talents with influence and respectability.—It were to be wished that they and other literary characters in town, had more concern for the improvement of the community at large, and would make suitable efforts to promote it.

It would be doing injustice to the liberality of the proprietors of the Athenæum and circulating library, to deny these institutions their respective merits and advantages. But I apprehend that neither of them is sufficient to supply the desideratum.

The Athenæum and circulating library.

sideratum mentioned by your correspondent. The first is principally calculated for the commercial part of the inhabitants, and those who have time to lounge; the second, although comprising much useful reading, is sometimes defective in the selection of the books, and affords little opportunity for the union of literary exertions.

Consideration in
favour of a pro-
prietary associa-
tion.

A society whose books are the property of the individual subscribers, is far better adapted, not only for advancing knowledge and bringing useful talents into notice, but also for giving a favourable bias to the pursuits of ingenious young men of all descriptions, to whom such a society is at all times accessible, from the small expence attending it. People become more solidly concerned in promoting the success of any scheme, in proportion as their personal interests are interwoven with it; and we may therefore conclude, that a man will take more pleasure, and perhaps derive more profit, from reading a book which he considers as his own property, than one only lent him for a time.

Subscription-
library establi-
shed Feb. 1805,

Impressed with these considerations, a few persons in Aberdeen instituted a subscription-library upon the 22d February, 1805, under the title of the *Caledonian Literary Society*. Besides embracing all the periodical publications of merit in Great Britain, our stock is enriched with a selection of the most approved books, either presented by the members or purchased from the Society's funds: Which Society has *already* increased to upwards of 100 members, and the list is daily augmenting in number and respectability.

at a very mode-
rate expence.

It is worthy of remark, that the trifling sum of six shillings *per annum* is only required from each subscriber to *The Caledonian Literary Society*. So inconsiderable an expence, contrasted with the great variety of useful and entertaining knowledge to be derived from it, must form a very powerful recommendation in its favour.

We have been informed with pleasure, that many persons in Glasgow, who are not members of the Society established there, have contributed liberally to its support by giving books — an example worthy of the imitation of others.

A Philosophical
Society in con-
templation.

It is also in contemplation to institute a Philosophical Society, on a plan similar to those of London, Edinburgh, &c. for the purpose of receiving occasional dissertations on a variety of literary

literary and other subjects, to be deposited as the property, or, entered into the books of the society; and afterwards published in such manner as the society may direct.

Should any of the friends of science in Inverness, Banff, Peterhead, or other places, be desirous of establishing similar institutions, we will most cheerfully furnish them with a copy of our plan and regulations.

We have a sincere wish to see every encouragement given to undertakings so laudable and beneficial, and have with this view made the present communication, to give publicity to ours through the medium of your excellent Journal. The insertion of the above will oblige, Sir,

With respect,

Your humble servant,

ALEX. CROMBIE, *Pres.*

Aberdeen, January 2, 1806.

XIV.

Letter from Mr. JAMES STODART, in Answer to a Question concerning the Effect of the Nitrous Oxide, proposed by Dr. Beddoes.

To Mr. NICHOLSON.

DEAR SIR,

DR. Beddoes, in a paper on the medical effect of respiring the nitrous oxide, published in the last number of your Journal, refers to an account I formerly gave of some unpleasant and rather alarming sensations experienced after inhaling that gas. He attributes the whole to hysteria or nervous affection; at the same time signifying a wish that I would state whether or not that was really the case. In answer to this I have only to observe, that if any such predisposition to hysteria did exist, it was wholly unknown to me. My general state of health was as usual; nor had any thing occurred particularly to affect the mind. I had often inhaled the nitrous oxide under circumstances in every respect similar (at least as far as I can judge) and till that time, so far from experiencing any thing like

Qu. Whether Mr. Stodart was nervously affected previous to his feeling inconvenience from nitrous oxide.

Reply: that he was not.

like debility, the very contrary effect was produced; namely, sound and undisturbed sleep in the night, followed by strength and increased cheerfulness on the following morning.

Expectation that the nitrous oxide may prove eminently useful, &c.

I very sincerely hope the medical application of this extraordinary agent, directed as it is by the very able hand of Dr. Beddoes, may prove as important and useful in medicine as it is interesting and curious in philosophy.

I have not yet heard of its being tried in cases of suspended animation; it appears to be an experiment well worth making. The subject is perhaps worthy of the attention of the Humane Society. I am with respect,

Dear Sir,

Your's sincerely,

JAMES STODART.

Strand, January 22, 1806

XV.

Description of a Statical Lamp, which maintains a Supply of Oil to the Burner from a Reservoir, placed so low as to occasion no Interception of Light. By A. F.

To Mr. NICHOLSON.

SIR,

Description of a new statical lamp.

I SEND you a sketch of an overflowing lamp, of which the construction will be easily deduced from the figure. Its advantages are, that the flame is supplied from below, and the light is not intercepted, but falls on all surrounding objects as directly as that of a candle. The upper part of A (see Plate IV.) contains the usual apparatus of a lamp, either according to Argand's construction or any other; and the column or tube which supplies the oil may be no longer than that supply and the conditions of the structure may demand. The vase below contains the oil, which is poured in, when needful, at the top of the column, by a funnel or otherwise. The circle round B, C, represents a globular (or cylindrical) vessel, having no communication with the vase except through a neck

a neck or pipe D, proceeding downwards nearly to its bottom ; but there is a communication with the external air, through a perforation (represented by a small shaded circle near B) which prevents the atmosphere from interrupting the intended action. The lightly shaded semicircle B represents an hemispherical solid capable of revolving on an horizontal axis, so as to hang downwards and fill the lower half of the globe, when no fluid is present ; or it can be raised up by floatage into any other position, according to the quantity and density of any fluid that may be poured in.

Description of a
new statical
lamp.

Let us now suppose the vessel C to contain any fluid not more than half its capacity, and that the revolving piece B is of such a weight as to be of half the specific gravity of that fluid : it may then be easily understood that the piece B will settle into such a situation as that part of it shall be immersed in the fluid and support it in the vessel, exactly to the height of its axis. For the part of the solid, immersed on one side, is exactly equal to the space above the fluid in that situation, on the other side ; and the greater part of B which is on one side of the perpendicular will exceed the smaller part on the other side, by exactly double that quantity. Consequently the immersed part of the solid will be pressed down by twice its own weight ; and this is exactly equal to the weight of fluid which it displaces ; whence the body and the fluid will be in equilibrio. Let us now suppose the fluid to be brine, at the specific gravity of 12, which may be poured in either at the top or at the side hole, and that oil of the specific gravity of 9 be then poured upon it ; and it is manifest that the oil will press the dense fluid upwards into C, as represented in the figure, and that when C is half filled, the oil will stand at an elevation above the axis equal to one half more than the height of the dense fluid, measured from its surface where the oil presses upon it. And, when this adjustment is once made, by putting in the proper quantity of dense fluid, if any of the oil be taken out, or consumed by burning, the pressure will be less, and the dense fluid will rise within the vase. But this rise will not be attended with any depression in the vessel C, because the level will be kept up by the revolving piece B, and consequently the oil itself will be prevented from falling as much as it would have done if this contrivance had not been applied.

Description of
a new statical
lamp.

I do not disguise the consideration, that as the oil diminishes, the distances between the upper and lower surfaces of the dense fluid must diminish, and a proportional difference or subsidence in the surface of the oil must take place. The proper remedy for this appears to be that the lower surface should be made as large as convenience will allow; that its rise and fall may be less.

With regard to the disposition and form of the spaces which are to contain the oil, it is only needful to observe that they may all be made small or narrow, except that which is alternately to be occupied by the oil, and the dense fluid. If the height of the dense fluid be 12 inches, the lamp may stand 18 or 20 inches high, using salt water as above mentioned.

There are various practical objections to mercury; but if this fluid were to be used, the oil might be raised ten times as high, or the apparatus, if required, might be constructed with a less distance between the surfaces.*

I am, Sir,

Your constant Reader,

A. F.

XVI.

Letter from a Correspondent rectifying some Particulars of Misinformation respecting the Fishery of the North of Scotland.

To Mr. NICHOLSON.

SIR,

WHEN any important information is communicated to the public, we have a right to expect that it should be given with extreme accuracy; or at least where any doubts exist, with such a degree of diffidence and modesty, as may leave room for avoiding misrepresentation or falshood.

* The contrivance for keeping a fluid at its level by a semi-circular revolving solid was invented by Robert Hooke. See Birch's History of the Royal Society. A. F. has ingeniously adopted it to a lamp which casts no shadow. Hooke's lamp is nearly as faulty as the common fountain lamp in this respect. N.

I wish

I wish an *Enquirer*, in your Journal for December last, had attended to this, before making what I conceive to be a hasty, ill-founded statement, respecting certain instances of wasteful negligence in some of our fisheries in the north of Scotland, which it is my duty at present to controvert.—He states :

1st, "That the fishermen of Aberdeen, Banff, Peterhead, &c. never think of carrying their fish along the coast southward, which they might do to Leith in twenty-four hours; or with a good brisk wind to Berwick-upon-Tweed, or even Newcastle-upon-Tyne; but when their respective towns are supplied, they throw the remainder upon the dunghill for manure!!"

A fact so improbable as the above, would indeed, require no ordinary share of proof to gain credit to it, and I have the satisfaction to assure you that it is entirely without foundation. The truth is, the number of hands employed in the fisheries in the north of Scotland are so few, and the encouragement given to enterprize and speculation in this important source of national wealth so small, that no more fish is caught than what supplies the neighbouring towns. But even admitting that more were caught, and that we could vend at Leith, Berwick-upon-Tweed, or Newcastle-upon-Tyne, is it not to be supposed that fishers of places nearest to these towns could afford to greatly undersell us?

When the dog-fish (*Jiquulus catulus*, L.) appear on the coast, our fishers catch a great number of them and dry them for their own private use (for none but themselves and the lower classes of people would use them) and likewise for the benefit of the oil, which they yield in great abundance, and the skin, which is used for smoothing the surface of wood. After they are drained of the oil which they contain, besides keeping a sufficient number for use, they throw the remainder on their dunghills, which produces a valuable manure. And no doubt your correspondent may have mistaken these for any other kind of fish.

He next observes; "That at Arbroath, another custom, equally as extravagant in its kind prevails, and of which I have been a witness; the crab fishery is so productive, that after boiling them, the bodies of the crabs are thrown away, and the large claws only brought to table."

It is

The claws of crabs only are sold at Arbroath, but the bodies are not thrown away.

It is indeed, generally the case here, and in every other fishing town, that the fishers for the most part retain the bodies of the crabs, and only dispose of the claws in the public market: but that the former are thrown away, is by no means true in almost any instance; for the fishermen find them of far more value in baiting their hooks, than what they could get for them otherwise. Indeed, if it were not for this purpose, it is believed, few or no crabs would be caught at all.

Much profit might be derived by a company if established at Aberdeen for exporting white fish.

Having thus endeavoured to vindicate our fishers from the charge of wasteful negligence, which none who know them will think them guilty of; I cannot conclude without expressing my surprize that no company has yet been established at Aberdeen for exporting white-fish. It is obvious from its excellent situation, and advantages, that very handsome profits could be cleared, if such an undertaking were once set on foot, and well conducted; equal, if not superior to the salmon fishing, which it is well known has been greatly the means of enriching this place.

If you deem the above observations worthy a place in your useful Journal you will oblige,

SIR,

Yours respectfully,

A. L.

Aberdeen, January 3, 1806.

XVII.

Observations and Enquiries concerning the Heat of Air blown from Bellows. By K. H. D.

To Mr. NICHOLSON,

SIR,

Passage from Dr. Black's lectures.

I BEG leave to mention a passage in Dr. Black's Lectures on the Elements of Chemistry, published by Professor Robison, which occurs at page 88. Vol. I.

The author is speaking of the communication of heat, and has, in the former part of the page accounted for the apparent coldness of a stream of air, by its preventing the accumulation

mulation of heat around our bodies, by its impulse and rapid succession, both cooling our clothes faster, and carrying away the warm air that was intangled in them. The Doctor says, "the sensation of coldness, therefore, produced by wind, or agitated air, is so much stronger than that produced by equally cold air in a stagnating state, that we are often persuaded the agitated air is actually colder, until we examine it by the thermometer; and Dr. Boerhaave thought the deception so strong, that he contrived an experiment to remove it completely (Boerhaave *Elementa Chemiae*.) He suspended a thermometer in the air of a large room for some time, and noting the degree to which it pointed, he then directed against the bulb of it a stream of air impelled by a large bellows in the same room;—that stream of air would certainly feel to a person who opposed any part of his body to it, considerably colder than the rest of the air in the same room; *but the thermometer is not in the least affected by it.* And it would be easy to exhibit another experiment to shew, that agitated air is not made colder by agitation. A piece of ice, for example, being suspended in the air of a warm room, and blown upon by bellows, instead of being thereby kept the more cool, as our hand would be, and preserved the longer from being totally melted, would certainly be melted so much the faster, than when the air is allowed to stagnate in some measure around it."

that agitation of the air, though it cools heated bodies, does not render the air colder,

nor hotter, though it melts ice.

I take the liberty of troubling you with this in consequence of a communication from your ingenious correspondent, Mr. Richard Winter, published in the last Number of your excellent Journal, where his experiment on the effect produced on a thermometer by a blast of air from a pair of bellows, directly contradicts Dr. Black's assertion, that "the thermometer is not in the least affected by it."

M. Winter found the air from bellows gave out heat.

That there is great truth in Dr. Black's general statement of the fact, of a blast of air cooling a body warmer than itself, by affording a continued series of fresh surfaces to carry off the caloric, I have no doubt, and that it should have an equal effect in warming a body colder than itself, seems equally evident, or by supplying the colder body with caloric. But in the case of the thermometer being raised four degrees, (as stated in Mr. Winter's experiments) we are not told that it was of a temperature lower than that of the air of the room

Questions respecting these facts.

room. How then, Sir, are we to reconcile the result of your correspondent's experiment with Dr. Black's assertion, mentioned above?—Are we to suppose the blast of air to have actually acquired an increase of temperature, and if so, how has it acquired it? I hope your correspondent (should this ever reach his ears) will not imagine I doubt the accuracy of his experiment; my only object is, the clearing up a circumstance, which at present is to me at least, not by any means satisfactorily accounted for. To whom then can I better apply, than to you, if indeed I may venture to hope you may think the object worthy of your consideration? Whether that shall prove the case or not, I must always feel (in common with thousands of others) the benefit you confer on the scientific world, by the easy means of communication of knowledge to the public, which your Journal affords,

I have the honour to be,

Sir,

Your obedient Servant,

K. H. D.

Tunbridge,

January 19, 1806.

P. S. I do not understand how the supposed greater capacity of a vacuum for caloric explains the facts, whether of the rise of the mercury in the thermometer, or the melting of the ice.

Observations on the preceding Letter, by W. N.

It is desirable that the experiments should be repeated.

WHEN a question arises concerning the disagreement of facts, the process obviously indicated is to repeat the experiments; in order that it may be seen what circumstances may have tended to produce mistake, or what may have been the real difference between operations supposed to be the same. Though I have not had an opportunity of doing this, I have nevertheless thought it proper to make a few remarks.

Agitation enables a fluid to gain the common temperature more speedily.

When a body is immersed in the air, or in any other fluid differing from itself in temperature, the body will acquire the common temperature more speedily (that is to say, it will be heated or cooled more quickly) by agitating the fluid, than if it were left undisturbed;—and this for the plain reason, that more of the particles at the original temperature will come into

int contact with it in the latter than in the former case. These remarks support and explain the facts noticed by Dr. Black and Boerhave. Agitation of the air is merely supposed, and not that it shall be either condensed or rarified. Many facts concur to shew, that the capacities of elastic fluids for heat are increased by rarefaction, and diminished by condensation; proofs of which we have by experiments in the air-pump and condenser, and in the late experiments of explosions produced in the chamber of the condensing syringe. If we attend to this law, we must infer that the air in a pair of bellows, being suddenly compressed by a force perhaps equal to one twentieth of an atmosphere or more, will acquire an increase of temperature; and if in this disposition to give out heat, it be made to rush against the ball of a thermometer, it will heat the mercury, and cause it to rise in the tube. Now, in order to reconcile both the results of Mr. Winter, and of Boerhave to truth, we must recollect that bellows, like the unfortunate traveller in Esop's Fables, can blow hot and cold at the same time. If the thermometer be held very near the aperture, the warm air will heat the mercury; but if it be held at a greater distance, where the warm air has become plentifully mixed with cold, the effect of its temperature may be altogether inconsiderable, while that of the agitation continues to be effective: that is, to say, the thermometer if already at the common temperature, will neither rise nor fall; if it be already hot the steam will cool it; or if cool the steam will heat it. Thus it is, to return to our traveller, that we breathe upon our fingers held close to our mouth when we mean to warm them; but when we wish to produce cold, we hold the subject at a distance, and blow at it.

Air is heated
by condensation.

Whence the
rear blast of bel-
lows will be hot,
and the remoter
will act only by
its motion.

As the thermometer falls in the pneumatic vacuum, I suppose there may be some mistake in the postscript.

XVIII.

Account of the Performance of the patent Ship Economy at Sea, in a Voyage to the West India Islands, and of some Improvement in the Tackle aboard, proved of great Utility. By Mr. J. WHITLEY BOSWELL.

To Mr. NICHOLSON.

DEAR SIR,

Description of the ship's construction has been published in a former number of this work.

AS in a former number of your Journal *, you favoured me by inserting a description of the construction of the ship Economy, built according to my patent, I hope you will also admit the following account of her performance at sea, and of some other matters; of considerable utility to naval concerns.

The subject proper for the Journal as containing an account of an experiment in the arts important to the nation, and on a great scale.

Your Journal is principally devoted to the furtherance of the most useful of all knowledge, that of experiments in Philosophy and the Arts. And to a nation which like this depends on its shipping for most of the many advantages it enjoys over the rest of the world, what experiments can be more important, or ought to be more interesting, than those which concern this subject?

Gentlemen who have assisted in this experiment.

The experiment which has been made on this occasion is entitled to a farther superiority over other usual experiments, an account of the large sum of money required for conducting it, which altogether rather exceeded 5000*l.* and on this occasion it is but justice to mention the spirit with which Wm. Lushington, Esq. of this City, and Richard Griffith, Esq. of Dublin have come forward to assist in making this experiment, whose property the ship principally is, (my share of it being comparatively small to theirs); to those gentlemen this country is chiefly indebted for proving a matter of great utility to its naval concerns, and which sooner or later must be of the greatest advantage to it, when the plan comes into use, though the spirit of the times may defer this period until it shall cease to be of any benefit to us, and others may reap the profit of these gentlemen's public spirit and my labour and study; but as I waited till I should have the proof of actual experiment to add, to that of a theory (which though founded on un-

The plan must be of great use to the nation when adopted,

erring principles, and of which each part had been often proved in detail before, it could not be expected to convince those whose pressure of business, or want of taste for such studies, deprived of time, or inclination, or made it too great a labour to attend to its demonstration in any other way) I shall hope now, (that my exertions to bring this plan of ship-building into the notice it deserves, when its sufficiency, strength and security is,) supported by actual and severe proof, will meet with a fair and candid consideration, from the direction of our navy, and those whose commercial pursuits lead to employ vessels of great burden.

The chief advantage of this method of ship-building is, that it enables the builder to use timber of much less cost, and vastly more easy to procure, with strength and stability superior to the old method, in proportion to the quantity of timber, and to dispense with knee timber entirely.

In a national point of view this method is still of greater benefit; for as it admits of timber of fifty years growth to supply the place of that of one hundred, not only the forest lands may be made to produce timber for double the number of ships for our navy in a given time, but private gentlemen would be also induced to plant more timber for this purpose, from the superior profit they could in this case make of their plantations, and the hope it would give them of being able to receive the fruits of their labour during their own lives, which at present can only be expected to be reaped by their grand children.

An oak of fifty years growth has also a much greater quantity of serviceable timber in it, in proportion to its age, than one of an hundred years, and four times the number of them at least can stand and flourish at one time at the same extent of ground; so that the public would be benefited by the adoption of the plan every way; for while timber would thus be rendered more plenty, those who prepared it for market would also obtain a greater profit.

Hitherto the price of timber for the navy has been attempted to be kept down by arbitrary regulations, which tended to encrease its scarcity; at last, notwithstanding every effort, the price and scarcity have encreased so much that our government have been forced to the expedient of partly relying on a foreign country for the continuation of the navy; and to

I hopes that the success of the experiment will accelerate this period.

Economical advantages of this method of ship-building, cheaper timber, more easily procured, may be used in it.

Its national advantages, the forests could supply double the quantity of the timber wanted in this plan in a given time, more timber would be planted if it was adopted; oaks of fifty years, have more serviceable timber in proportion than those of 100, and occupy only one fourth the ground.

The scarcity of ship timber has compelled this country to build ships of war in Russia; danger of this expedient.

depend on the dock yards of Russia for the bulwark of the British nation, for the defence of its liberties, and of its political existence, and this at a time when our crafty and implacable foe has got possession of nearly all the forests of the rest of Europe, and is making the most prodigious exertions to out-number our navy.

Should induce the trial of the patent plan, in doing which there is no risk, as its sufficiency has been proved.

If my plan of ship-building tends in to great a degree to diminish those difficulties, and even dangers, as is stated above, is it not worthy of a trial at least, even if some risk was run in that trial? but when no risk is run, when the plan has been proved, the most scrupulous economist of the public wealth can start no objection to that trial of it in the navy, that the public necessity for some expedient to supply timber for its use so loudly calls for.

No public money required to make experiments, they have been already made at the expence of the owners of the ship.

We ask no drafts on the public source to try experiments on the subject, these have been already compleatly made at our own expence, and all we demand is our country to condescend to reap the fruit of our exertions; if she does, we shall rely on her generosity to recompence us, convinced that she will have ample proof that we have deserved it; but should this not be the case, we will not rest contented with having discharged our duty, in doing the most we could to serve her; which if we should be so happy as to effect, we will never regret our trouble or cost.

The performance of the ship at sea cannot be mistated on account of its publicity.

Having thus stated the claims which the subject has to public attention, I shall proceed to relate the performance of the ship at sea, which, as she sailed in company with a large convoy both out and home, is a matter of too public a nature to admit any misstatement I might wish to make, which God knows is far from my desire.

On the 22d of August, 1804, The patent ship Economy weighed anchor off Gravesend, with but a small cargo aboard, as is usual for ships outward bound to her destination, and set sail on her voyage to Trinidad and Grenada; and on the 14th October following arrived at Grenada; her performance on this voyage is best stated in her Captain's own words, in the following extract from a letter to Wm. Lushington, Esq. London.

SIR,

Grenada, Oct. 15th, 1804.

The Captain's letter relative to the voyage out.

I have the pleasure to inform you of the ship Economy's safe arrival here yesterday evening. We had a fine passage, and

and had but one gale of wind: The ship performs as well as it is possible for a ship; is remarkable easy at sea, steers and sails well, and is perfectly tight. In the gale of wind the Epervier man of war (sprung her foremast); the Robert Aylward ditto; a brig, Master or brig named Swinger, lost both top-masts and parted her mavy in lat. 14. 30 N. Our ship behaved extremely well and never strained a rope yarn.

Ship steers and sails well, is remarkably easy at sea, perfectly tight in a heavy gale, in which other ships suffer much, she meets no accident:

(Signed) ALEXANDER SMITH.

From the period of this letter she remained at the West India islands until the 23d of July 1805; being detained there the greatest part of that time by the arrival of the French fleet, which was afterwards chased back to Europe by the gallant and ever to be regretted Lord Nelson; from the 23d. of July, when she sailed for England, to the 29th of Oct. when she cast anchor off Portsmouth on the Mother-bank, she experienced a series of severe weather, and violent gales of wind, in which some of the fleet with which she returned foundered, and others were obliged to bear away for America for shelter. The remarkable bad passage home of the Leeward island fleet, of which she was one, is too well known to need much description: all seamen must be sensible that three months tossing on the Atlantic ocean in such hard weather, beating up against contrary winds, to a vessel as deeply laden with sugar as the skrews could compress it into her, must have been a most severe trial, and that if she had a single weak part, or defective principle in her construction, it must have given out in that time: but while most of the other ships of the fleet met with more or less damage both to themselves and their cargoes, she bore through all without the smallest accident, and brought home her sugar perfectly dry and safe; which was not completely discharged until Jan. 1806 (on account of her detention at Portsmouth, through contrary winds from whence she she did not get to London before the 27th. of Nov. on which day she hauled into the West India dock,) or this account would have been made public before. A further proof of the stability of her frame work, is her taking the ground with a full cargo on board without any accident, as may be seen more particularly in the following account of her performance home which I received from her Captain.

delayed at the West Indies by the French fleet,

returns home, experiences violent gales, and a tedious passage of three months, some ships of the fleet founder through severity of weather. Why this severe trial is a sufficient proof of her stability, when deeply laden,

took the ground without injury at Trinidad.

DEAR

DEAR SIR,

January 17, 1806.

"IT is with pleasure that I have pleasure to inform you of the performance of the patent ship Economy, during the voyage under my command.

Captain's account of the voyage home, In a violent gale the ship performs extremely well, and is a good sea boat: two ships foundered in this storm, one abandoned, another large ship rendered unmanageable, and taken in tow, and several others much damaged.

The Economy meets no accident, and is very weatherly; tho' deep laden; has a foul bottom which impedes her sailing, she works and steers well.

She remains perfectly tight after the severe passage, though run aground at Trinidad with a full cargo of sugar;

is a very remarkable strong ship,

"On the fifth and sixth of September last, latitude 37° 34' N. we experienced a very heavy gale of wind, with an heavy cross sea, occasioned by the wind shifting to different points of the compass suddenly, and blowing with extreme violence: during the whole of the gale, the Economy behaved as well as I ever experienced a ship to do, and much better than could have been expected for so small a ship; in fine, she is as good a sea boat as ever put keel in salt water. During the gale, two ships, it is supposed, foundered; after the gale one was abandoned as not tenable, should another gale of wind come on: the Prince of Wales, a ship of 300 tons, had every thing washed from her deck: The Princess of Wales, a ship of the same size, broke her rudder, and was left in tow of the Hyena sloop of war. Several other ships met with considerable damage, which proved undeniably the violence of the wind. Notwithstanding the lumbered state of the Economy, we lost nothing off deck, and I don't think there was a ship, large or small in the fleet, that made better weather: she did not sail so fast coming home as going out, but that is easily accounted for, when we consider she was not coppered, and was out fifteen months on a wooden sheathing, with barnacles as long as your finger on her, and the bottom resembling a rock; and was besides laden as deep as she could stow. She works and steers amazingly well. I would not wish to change her if she had been larger, but being only 200 tons, she is too small both for my interest and the West India trade.

"The ship has been perfectly tight all the voyage, although we had a very tempestuous passage, and likewise ran her on shore, *sugar loaded*, under the batteries at Trinidad, to prevent her falling into the hands of the French, as we supposed, where we lay for twenty-four hours, until we discovered that it was Nelson's fleet. In my opinion she is one of the strongest ships in the river Thames of her size.

"The new iron slings and other iron work on the yards exceed my most sanguine expectation, I have seen the ship covered with flashes of lightning when at Trinidad, and never experienced

experienced the least injury from so much iron being about the yards, owing to the precaution which I took of serving the iron work and paying it with pitch, which I think served as a non-conductor. I have a higher opinion of iron work than ever I had, and think the iron rigging in the plan we used to talk about while the ship was building, would answer to admiration, and might be the means of preserving the masts of men of war, when in action, as being less liable to be cut with shot. When I can manage it, I mean to rig the mizen mast of a ship wholly with iron, to give it a trial. When I examine the bottom, I will give you my opinion of the pieces of sheathing steeped in your preparation to prevent the worms from destroying the bottom. The large rollers which you had let in beneath the hawse holes for the cables to work on, were of very great benefit, and I think saved us the labour of two men in weighing anchor, they also prevented the wear of the cables very much, and were greatly liked by the sailors, as making the purchase more lively,

of the iron slings to her yards, and captain's high opinion of the use of iron work in rigging. State of experiment on part of the sheathing will be attended to, great use of the large rollers for the cables.

Your's very sincerely,

ALEX. SMITH."

The iron slings which Captain Smith mentions, were on a plan of his own, and different from those used in men of war, in not requiring above three or four feet of chain for each yard, and served merely to suspend the yards from the point of the tops; which method greatly saved the wear of the masts, and permitted the yards to work more freely. Iron straps were also used to most of the blocks instead of hemp.

Further account of the iron slings,

The rollers for the cables were about fourteen inches long and eleven in diameter, and worked on iron gudgeons about two inches in diameter, in brass sockets. The rollers which have been hitherto used for this purpose, were generally much too small, seldom exceeding the diameter of the cable; which diminished size both increases the friction and injures the cable, from the smallness of the nip which they occasion; or, in other words, from the acuteness of the angle at which the cable is forced to bend in passing over them.

and of the cable rollers,

In concluding this account I beg leave to mention, that I could, in building another ship, greatly diminish the space necessary for the transverse forms used in my plan, by setting them farther asunder, and forming them of iron, which

Iron transverse frames may be used in future in this place, to save room.

method

—and the fore
and aft ribs
scarped in a more
economical
method.

method is specified in my patent, and that I could also make a great saving in the timber used in the fore and aft ribs, by a method of scarping them, also within the limits of my specification. Experience has since convinced me of the superiority of both these methods, of which I had some doubt when I built the Economy, or they should have been used in her.

It may seem paradoxical to assert that iron is oftentimes cheaper than wood in ship building, when it can be used: but a plain proof of this exists in the bow of the Economy, of which the three lower breast-hooks are iron of considerable substance, and yet cost less individually than any of the wooden ones above them, though these are of no extraordinary girth, or of much curvature.

The Economy
will remain a
few weeks at
the London
Docks for in-
spection.

The Economy will be a few weeks in the London Docks, where she has now moved, for the inspection of the public, and where all gentlemen who are interested in shipping concerns may see her construction; and those who examined her previous to her sailing, may convince themselves that I have exaggerated nothing, as to the sound state in which she has returned from her tempestuous voyage.

Dear Sir,

Your very humble servant,

J. WHITLEY BOSWELL.

XIX.

Experiments on the Torpedo. By Messrs. Humboldt and GAY LUSSAC. Extracted from a Letter of M. Humboldt to M. Berthollet; dated Rouen, 15 Fructidor, Year 13 (Sept. 2, 1805.)*

THE curious theory with which Volta has enriched the science of natural philosophy, on the subject of electric fish having been received as authentic by many naturalists, renders the phenomenon of the Torpedo worthy of further investigation. You know, my dear friend, what was our impatience to procure these fish, and will perhaps be surprised that so much time should elapse without having heard from us on the

* Annales de Chimie, Vol. LVI.

Subject. At Genoa, we perceived some; but we were then without our instruments. At Civita Vecchia we sought them in vain. But during our stay at Naples we frequently procured some very large and lively ones. In this letter you will find detailed the experiments made by M. Gay-Lussac and myself on the powers of this fish (*Raja-torpedo* of Linneus). M. de Buch, a German mineralogist, well acquainted with all the branches of physical science, was witness to our proceedings. I sent you the results, giving simple facts, unmixed with theoretical speculations. Our experiments were chiefly directed towards the discovery of that state of the torpedo when it was least capable of exerting its power upon the human frame. This power has been generally described as electrical; but the sensation produced by it is materially different from that caused by the discharge of a Leyden phial — Having no other book by us besides the work wherein Aldini* combine the researches of Geoffroy with those of Spallanzani and Galvani, it is not to be expected that we should compare our experiments with those which may have been previously made by other philosophers.

The torpedo found at Genoa and Naples, but not at Civita Vecchia.

The shock of the torpedo feels different from that of electricity.

1. Though the strength of the torpedo is far inferior to that of the gymnotus, it is equally capable of causing painful sensations. A person much accustomed to electric shocks, can hardly sustain that of a lively torpedo of four decimeters (16 inches) in length. The animal acts under water, and it is only when it loses strength that the fluid impedes its action. In this case, M. Gay Lussac observed that the shock is not perceptible till the fish is raised above the surface.

Powers of the torpedo inferior to those of the gymnotus of S. America. Shock of the torpedo more violent than that of electricity. It acts under the water,

2. I observed, when in South America, that the gymnotus gives the most violent shocks, without any exterior movement (of the eyes, the head, or the fins: it appeared as tranquil as a person when passing from one idea to another, or from one sensation to another. Not so the torpedo: We observed a convulsive movement of the pectoral fins, each time it gave a shock, which was more or less violent according as the surface was larger or smaller wherein the contact took place.

—and seems to use more effort than the gymnotus.

3. The powers of the torpedo and gymnotus cannot be excited at pleasure, as we should discharge a Leyden phial or a

Shocks from the torpedo and gymnotus cannot be obtained but by irritating the animal.

* *Memoires sur la Torpille, dans l'Essai sur le Galvanisme*, Vol. II. p. 61.

conductor.

conductor. A shock is not always felt on touching an electric fish; it must be irritated before it will give the shock. This action depends on the will of the animal, whose electric powers perhaps, are not kept constantly charged; yet it can recover them with wonderful celerity, as it is capable of giving a long succession of shocks.

The shock obtained by a mere touch with the finger,

4. The shock is felt (the animal being disposed to give it) as well on touching with one finger a single surface of the electric organs, as on applying the two hands to the two surfaces, the upper and under, at once. In both cases it is immaterial whether the person applying his finger or his two hands, be insulated or not.

—but the contact must be direct. Metals seem to be non-conductors of the shock of the torpedo,

5. When an isolated person touches the torpedo with a single finger, it is indispensable that the contact be immediate, as no shock will be felt if a conducting body (of metal for example) be interposed between the finger and the organ of the fish.— For this reason, the animal may be touched with impunity by means of a key, or any other instrument of metal.

6. M. Gay-Lussac having made this important observation, we placed a torpedo on a metal dish, with which the inferior surface of its organs were in contact. The hand which supported this dish experienced no shock, whilst another isolated person irritated the animal, whose convulsive movement of the pectoral fins indicated a most violent emission of the electric fluid.

Experiments which shew that they conduct.

7. When on the contrary, a person held the torpedo in a metal dish in his left hand (as in the preceding experiment), and with his right touched the superior surface of the electric organ, he experienced a smart shock in both arms at the same moment.

8. The same was felt, on placing the fish between two metal plates, whose edges were not in contact with each other, and applying the two hands at once above and below them.

9. But if the edges of the metal plates be suffered to touch each other, no shock will be felt in either arm. The communication between the two surfaces of the organs is, in this case, formed by the plates; and the new connection arising from the contact of the two hands with the plates is without effect.

The organs of the torpedo have no influence on the electrometer.

10. The most sensible electrometer manifested no electrical tension in the organs of the torpedo; in whatever way it was applied, it was not in the least affected; neither, on directing it

it towards the organs, nor in insulating the fish, covering it with a metallic plate, and making a communication between this plate, by means of a conducting thread, and the condenser of Volta, was there any indication (as with the gymnotus) that the animal affected the electric intensity of surrounding bodies.

11. As electric fish, when healthy, exercise their powers as forcibly beneath the water as in the open air, we were led to examine the conducting properties of this fluid. Several persons formed a chain of hands between the superior and inferior surfaces of the organs of the torpedo: the shock was not felt until they had wetted their hands. The action was not intercepted when two persons supported the torpedo with their right hands; and instead of holding each other's left hand, they each plunged a metallic rod into water placed upon an isolated body.

Examination of the conducting powers of water.

12. By substituting flame in lieu of water, the communication was destroyed, until the rods touched each other in the flame.

Flame does not conduct the shock.

13. It must, however, be observed, that in water, as in air, the shock was not perceptible without an immediate contact with the body of the electric fish: the least possible intervention of the water prevented it. This fact is the more remarkable, as it is known that in galvanic experiments, where the frog is immersed in water, it is sufficient to direct the silver forceps towards the muscles to cause a contraction, though a body of water be interposed, equal to one or two millimetres in thickness, or about one-twentieth of an inch.

No shock can be obtained without immediate contact with the fish.

These, my dear friend, are the principal observations which we have made on the torpedo. The experiments, No. 4 and 10, prove that the electric organs of these animals are not susceptible of any intensity or excess of charge. Their action may rather be compared to that of a combination of Leyden phials, than to the conductor of Volta. Without communication no shock could be felt: and having experienced the power of the gymnotus through very dry cords, I imagine, that where I have been affected by this powerful animal without direct contact, it had been occasioned by some deficiency in my insulated state. If the torpedo act by poles, that is by an electric equilibrium which possesses a tendency to replenish itself, experiments 5 and 6 seem to prove that these poles exist near

Organs of the torpedo not susceptible of any excess of charge.

Doubt whether the shock of the gymnotus can be felt without actual contact with it.

Torpedo supposed to act by an electric equilibrium, the opposite state being very near.

Objections to
this notion.

Considerations
of theory.

each other, on the same surface of the organ. The shock is felt on merely touching the surface with the finger. A plate interposed between the hand and the organ, (*Exp. 6.*) re-establishes the equilibrium, and the hand which sustains the plate is not affected, because it is placed beyond the current. But if we suppose an heterogeneous number of poles upon each surface of the organ, whence does it arise, that, in covering these surfaces with two metal plates, whose edges do not touch each other, and placing the hands on these plates, the equilibrium should be found in the arms? Why, it may be asked, does not the positive electricity of the inferior surface seek at the moment of explosion the negative electricity of the next or nearest pole, but rather seek it in the superior surface of the electric organ? Perhaps these difficulties may not be insurmountable; yet the theory of these *vital actions* well deserves attentive research. Geoffroy has proved that thornbacks, who give no signs of electricity, are furnished with organs analogous to those of the torpedo. The least injury on the brain of the torpedo destroys its electric powers. The nerves are no doubt concerned chiefly in these phenomena; and the physiologist who should admit the power of vital actions, might with success oppose the theory of the naturalist, who would endeavour to explain all by the contact of the albumino-gelatinous pulp of the nervous laminæ wherewith nature has endowed the organs of the torpedo.

SCIENTIFIC NEWS.

Prizes proposed by the University and Academy of Witna, in June, 1805.

CLASS OF SCIENCE AND MEDICINE,

First Prize.

To determine whether saccharine secretions take place in other organs besides those affected in diabetes mellitus.

BESIDES the diabetes mellitus of the authors on medicine, are there any other disorders peculiar to man, which, according to experiments well ascertained, produce in different organs a secretion similar to sugar, sufficiently abundant to finally occasion consumption? And what are these disorders?

In

In a note on this subject it is recommended to examine for saccharine matter, the fluid substance of colliquative sweat, that produced in the *fluxus caliacus*, and in the pituitous consumption from lung, which after death are not ulcerated; and the milk of women afflicted with the *galla cirrhea*.

Second Prize.

What are the true characters and the causes of the malady, To ascertain the which although not exclusively appertaining to Poland, is true cause and however called the *Plica Polonica*? Are there any means of cure of the *Plica Polonica*. curing this disease more successful than those hitherto employed? and what are these means?

Third Prize.

What are the principal maladies of vegetables? And what is the true analogy between them and those of animals? Relative to the disorders of vegetables.

CLASS OF NATURAL PHILOSOPHY AND MATHEMATICS.

Prize.

Suppose a canal, through which a certain quantity of water m , flows in a given number of seconds, through a transverse section of a given depth and breadth, terminated by the two Banks. If on this section a dam is constructed, at the top of which an opening is made for the water to pass, of given dimensions; it is demanded according to what law the water, elevated by the obstacle which the dam presents, will be forced to rise not only at the dam, but backwards along the canal. The law is demanded, by which water rises in a canal behind a dam, at the top of which a given opening is made.

Formulae are required sufficiently general, to be applied not only to the quantity of water m , but to any other $m + 2$. Experience not exactly agreeing with the theory, the necessary corrections must be made to the formulæ, and proofs given from facts and observations, shewing how nearly they approach the truth.

CLASS OF MORAL AND POLITICAL SCIENCES.

Prize.

As the sciences of natural philosophy and mathematics make daily advances, and are enriched with new discoveries, it is demanded—

Qu. why moral sciences do not make the same progress as the physical?
 —If they can be farther improved?
 —What are the bounds to their perfectibility?
 —What are the best methods to attain this point?

1st. Why the same does not take place in the moral sciences?

2d. Whether among the different branches of these sciences, there be any capable of a farther degree of perfection? And what these are?

3d. To what degree are they of this nature? And what are the limits to their farther improvement?

4th. What are the most proper methods to advance the moral sciences to this boundary of perfection?

It is desired that the discussion of this subject may be conducted so as to present results, which may contribute to the perfection of that theory of Legislation, which is most conformable to the nature of man.

Second Prize.

Tenets of Adam Smith and Dr. Quésnay?

To determine (by making an analysis of political economy) what are the points in which the leading notions of Adam Smith and Doctor Quésnay agree, and in what they differ, or are opposite?

This examination must necessarily produce results useful to the progress of political economy.

Amount of prizes, and last days for receiving memoirs.

The prize for each of these questions is 100 golden ducats of Holland (46l. 5s.); and the last day for the reception of memoirs on medical subjects, the 31st August, 1807; and for the others, the same day and month in 1806.

Conditions to be observed by the Candidates.

To each memoir sent in must be attached a separate and sealed note, containing the title of the work, and the name and address of the author: This note will only be opened by the University if the work shall obtain the prize.

Memoirs to be written in Latin, French, or Polish.

The memoirs must be written legibly, in Latin, French, or Polish languages. The packet should be addressed to the Rector of the University of Wilna, and addressed to one of the bankers of that city, MM. Keyser or Karner, that it may go free. The Rector will give a receipt to these bankers.

The University shall not be obliged to return either the memoirs or the drawings sent; but the authors will always be permitted to take copies of them.

Conditions relative to property, copy right, &c. of the memoirs.

The University engages not to print any of the works sent them, without permission of the authors; but the authors may, at any time, print them if they think proper.

The

The distribution of prizes should take place before the termination of the years in which they are to be determined. The prizes adjudged shall be published in the gazette. Time of distributing prizes.

The author shall receive his prize from the administrative committee of the Imperial University of Wilna, either in person or by deputy. The prize will be at his option, either a gold medal or 100 golden ducats of Holland.

The Professors and honorary members of the University of Wilna, cannot be candidates for the prizes. Professors of Wilna not to be candidates.

Revised Precipitates from alkaline Solutions of metallic Oxides.

M. Klaproth, a little before his decease, discovered that the solution of the metallic oxides in the alkalis, are as easily precipitated in their metallic state, by the other metals soluble in the same alkalis, as are the acid solutions of these metals by phosphorus: He has made a very ingenious application of this process to the analysis of tin ores, according to the method which is described in his *Beitraege*. In this operation tungstein is separated from tungstate of ammonia, by the addition of zinc, in the form of black flakes. Alkaline solutions of metallic oxides precipitated, in the metallic state, by other metals.

Experiments on falling Bodies, by M. BENZENBERG.

M. Benzenberg, professor of physic and astronomy at Dusseldorf, published, some months ago, twenty-eight experiments made with balls well turned and polished, which were made to fall from a height of 262 French feet: At a medium they produced five lines of deviation towards the east, according to the determination of the plumb-line, and the theory gives four lines six tenths. These experiments were made in the coal-mines of Scheibusch; they are an additional proof, if it were necessary, of the rotatory movement of the earth, of which no one now doubts. The last experiments, made at Bologna by M. Guglielmini, gave nearly the same results. A falling body deviates four lines six tenths east, after passing through 262 feet.

Geography.

Great pains are taking in the construction of an accurate map of Holland. The same precautions have been used in this business as in the measurement of the degree of the meridian. M. de Zach has published in his Journal the chart of the triangles which have been completed: They are joined to those

those which M. Delambre made for the great meridian; and the distance from Dunkirk to Montreal has been taken for the first base. When the triangles are finished, a base will be measured towards the north, to serve for the verification of the work. The Batavian republic have entrusted the direction of this map to Colonel Krayenhoff.

Chart of the
White Sea, by
General Kautzoff.

Some months ago there appeared at Petersburg a very fine hydrographical chart of the White Sea, of which General Kautzoff is the author: Many naval officers have worked under his direction for four years, in collecting the materials necessary to compile this chart. The coast of the White Sea, of its gulphs, and of a part of the Northern Ocean, have been laid down trigonometrically. The depths have been carefully sounded; and six of the principal points of the coast have been determined by astronomical observations.

M. Lartigue's
map of America
in relief.

M. Lartigue having been engaged for thirty years in constructing, at the marine depot (of Paris), a large and beautiful map of America in relief, has at length completed it. It is said that the mountains, and the islands, and the tints of the sea, are all exhibited in a manner most capable of interesting those who make geography their study.

Expedition of
Capt. Lewis up
the Missouri.

Several months ago, Captain Lewis, in America, undertook to ascend the river Missouri, in search of a passage to the South Sea. Very interesting intelligence may be soon expected from this expedition.

Survey of
France.

The work of the government survey, or *cadastré*, of France, has proceeded with activity; 2000 persons are employed in it in the 108 departments.

Effect of Heat on Magnetism

Magnetism de-
stroyed at 700°
of heat.

M. Coulomb has published an interesting memoir on the effect of heat on magnetism. At 200 degrees of heat, two-fifths of it are destroyed, and the whole at 700 degrees.

*A. Lock of ten
Thousand Combinations.*

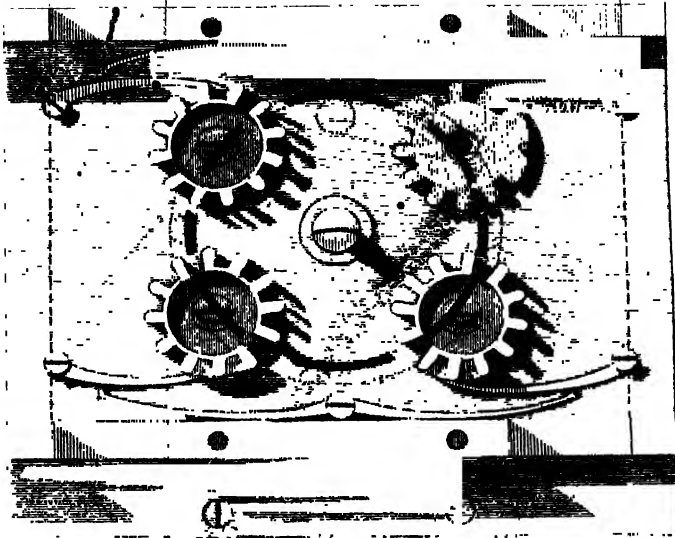


Fig. 1

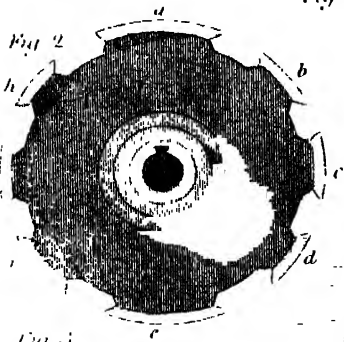


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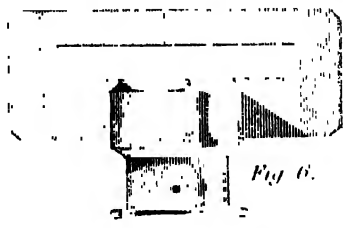


Fig. 6.

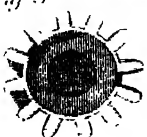
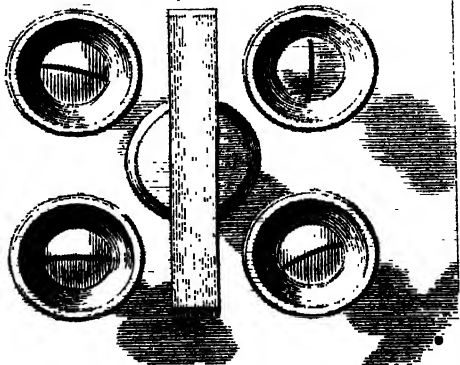


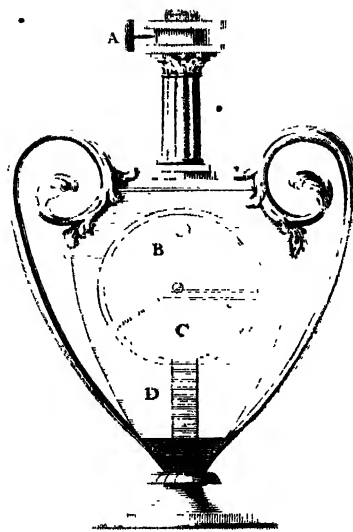
Fig. 3

Fig. 4.



Fig. 5





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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MARCH, 1806.

ARTICLE I.

Experiments on the Temperature of Water surrounded by freezing Mixtures. In a Letter from JOHN GOUGH, Esq.

TO MR. NICHOLSON.

SIR,

Middleshaw, Jan. 29, 1806.

MANY philosophers have turned their attention to the dilatation observable in water when cooled below 40 or 41 degrees of Fahrenheit's scale, and also to the no less singular fact of water retaining its fluidity for a considerable time when exposed to a freezing mixture, without being agitated. But one circumstance, relating to the latter phenomenon, appears to have escaped the notice of them all; which in all probability will prove of some importance to both enquiries.

We know from common experience, that when a hotter and colder body come into contact, the former will lose and the latter acquire heat, until they arrive at an equality of temperature. The frequent opportunities every one has of making this observation have authorised it to pass for a general rule; hence it has been concluded, that water in a state of rest may be cooled many degrees below the freezing point,

Expansion of water in cooling below 41 deg.

Explained by a development of the conjecture that ice may be formed in minute crystals at that temperature.

and still remain fluid. For my part, I adopted the maxim without hesitation, until the perusal of Dr. Hope's paper, given in the supplement to your last volume, led me to reason in the following manner on the subject.

When water is exposed to a freezing mixture, those particles of it which are in contact with the sides of the vessel, are soon reduced to a temperature lower than the point of congelation; in consequence of this, they are probably converted into minute icicles, which impart a quantity of caloric at the moment of their formation to the surrounding water, thereby preventing its temperature from sinking below 32° . These invisible bodies afterwards begin to ascend slowly on account of the diminution in their specific gravity; and while they rise towards the surface of the water, other particles will approach the sides of the vessel in succession, and undergo a similar transformation. This process would evidently increase the volume of the water without reducing its temperature, supposing it to be ice-cold at the commencement of the experiment; for the hypothesis rests on the supposition that water freezes as soon as it is cooled below the 32nd degree of Fahrenheit's scale. This gradual increase of bulk will explain the appearances described by my friend Mr. Dalton, who found that thermometers filled with water continued to rise when exposed to freezing mixtures, until the enclosed water congealed suddenly, and frequently burst his instruments. The reason why agitation accelerated the congelation of water thus circumstanced appears to be this: When the invisible icicles become very numerous, the least motion carries them in crowds against the sides of the vessels; where the small quantity of water contained amongst them crystallizes immediately, and cements the whole into a film adhering to the inside of the cup. This theory, or hypothesis, call it whatever you think proper, evidently requires water to be what it really is, namely, a bad conductor of heat; and after forming it, I proceeded to examine the merits of it experimentally, in the following manner.

Experiment.
Water at 32°
was cooled by a
surrounding
mixture, and
became con-
gealed at the
sides by stirring,
having never
sunk below 32° .

Experiment 1st. A small thermometer was suspended at the lower end of a wire, which could be moved in a vertical direction, through a hole in a horizontal bar of wood, fixed over a table for the purpose; a vessel, containing a freezing mixture, of the temperature of 21° , was next placed with

5

its

its centre under the wire; and the bowl of a wine glass, filled with two ounces of ice-cold water, being then properly placed in the mixture, the thermometer which stood at 32° , was immediately let down into the water, where it remained stationary for the space of seven minutes. A wire, cooled to the freezing point was now introduced into the glass, and the water agitated with it; upon which a thick coating of ice formed on the inside of the vessel; but no marks of congelation were observable on the wire or thermometer.

Exp. 2. The same apparatus being used, with a mixture, having the low temperature of 6° , the glass was filled with water of 58° , in which the thermometer fell to 32° in $7\frac{3}{4}$ minutes, by a stop watch; at which point it remained stationary five minutes longer. The glass was then taken out of the mixture, and the water being agitated, lined the upper part of it for about two-thirds of its depth from the brim, with a porous covering of ice, but the remaining part of it was free from all incrustation.

Exp. 2.
Water at 58°
was cooled by
an intense
freezing mix-
ture. It was
brought to sta-
tionary 32° and
sunk no lower.
When taken
out and shaken
the top froze.

I will venture to infer from the two preceding paragraphs, that we have all been under a mistake in concluding that water may be cooled when at rest many degrees below 32° of Fahrenheit, without congelating; at the same time we are certain, that it will preserve its fluidity, when judiciously exposed to great degrees of cold, and dilate at the same time, as Mr. Dalton has proved. Now as the heat never falls below 32° in these experiments, the expansion of the water in Mr. Dalton's thermometers, placed in a freezing mixture, cannot be ascribed to a loss of temperature, but must be owing to some other cause, probably to that which has been assigned above. As for agitation, the first experiment seems to shew its office to consist in bringing the water, crowded with minute icicles, into contact with parts of the vessel much colder than itself, where it is concreted into ice.

Exp. 3. To examine this part of the subject with more care, I formed a cup of caoutchouc, the capacity of which for caloric greatly exceeds that of glass; or, I believe, that of most other substances. Two ounces of water, a little warmer than melting snow being poured into this cup, it was placed in a mixture of the temperature of 15° , where it remained eight minutes without giving the least indication of a tendency to freeze. The cup was now removed from the

Exp. 3.
Repetition of
Exp. 2 very
strikingly in a
cup of caout-
chouc.

mixture, and gently shaken; upon which long icicles formed in an instant, projecting into the water in all directions, from the caoutchouc to which they adhered. This experiment, I have no doubt, might be made a very beautiful one by a dexterous operator, who is in the habit of exhibiting natural appearances to public assemblies.

After discovering that water will dilate without any change of temperature from warm to colder, at 32° , I began to imagine that the whole variation of expansion under 41° , might be explained on the same principle, because I believe all the experiments relating to the subject, have been made in a cooling medium, not warmer than melting snow.

Water expands by cooling between 41° and 32° deg. or begins to crystallize at the upper term.

In order to try the merits of this opinion, with an instrument larger than a common thermometer, I filled a four-ounce phial with water, and fixed an open tube into it, by means of a perforated cork and cement; but this apparatus proved my suspicion to be false. For the place of the water being marked on the tube when the temperature was 41° , my bulky thermometer rose immediately upon being plunged into water of 34° . This fact proves, that water expands by a loss of temperature between 41° and 32° ; or else, that this fluid begins to crystallize at the upper term; in consequence of which the lower term, or 32° , is not, properly speaking, the commencement of congelation, but the point at which the crystals of water begin to concrete into masses by aggregation.

I remain, &c.

JOHN GOUGH

II.

Account of the Art and Instruments used for boring and blasting Rocks; with Improvements. In a Letter from G. C.

To Mr. NICHOLSON.

SIR,

Bristol, Jan. 21, 1806.

Improvements in blasting mentioned.

BY way of appendix to Mr. Close's remarks on the use of sand in stemming mines in hard rocks, and his useful improvement of the pricker, by making it of copper instead of iron, allow me to add two other improvements in the art of blasting stone,

stone, which my own experience has proved to diminish considerably the expence of gunpowder, while one of them, at the same time, removes all danger from imperfect priming.

I shall also, with your permission, as many of your correspondents must necessarily be ignorant of the construction of the tools, give you a description of those now in use at the village of Shipham, in Somersetshire, a village wholly composed of men, women and children, who mine after lead ore, calamine, and ochre, chiefly in a lime-stone rock; a numerous band of some of the stoutest beings in England.

Description of the art as practised in the stone rocks in Somersetshire.

These men still use the iron pricker, because an accident seldom or ever happens to them; owing, I believe, in a great measure to their stemming with spar, and their habit of turning and loosening the instrument at every half inch they fill.

The iron pricker used without bad effect with spar for stemming.

The tools they use are these, *Plute V.*

A. A round bar of iron, bevilled off at one end, of 18 inches long, and of the diameter of half an inch.

Tools and implements described. A hole

B. A ditto, of 24 inches, to follow when the hole in the stone is about 12 inches deep.

eighteen or twenty inches deep and half an

C. A rod, with a loop for the finger, 25 inches long; at the bottom of which is a round flat plate of iron to draw out the pounded stone occasionally.

inch in diameter is cut by repeated strokes of a chisel, the edge

D. A pricker, 24 inches long, with a loop also, used to preserve a passage to insert the priming straw, while the hole is rammed or stemmed with E, the iron rammer, 20 inches long, and which, six inches or more from the end, is formed into a conical groove, very open at bottom, in order to enable the miner to ram round the pricker, and also that by its sharpness at the end it may the easier break to dust the pieces of spar dropped in as fast as wanted.

of which lies in the diameter of the hole and is shifted round between stroke and stroke.

E. A hammer with a handle and strap, about five inches long; the iron head weighing about four or five pounds, according to the strength of the operator; for some have them of six or seven.

They also have by them a bottle of water, to pour occasionally into the hole, for the wetter it is the faster the work goes on.

The work is performed wet, and the chipped stone scooped out with an instrument.

At every stroke of the hammer, the miner turns his chisel, by which means he works the bottom of the mine in a regular circle, and is enabled to keep his perforation true.

When arrived at the depth of 18 or 19 inches, he cleans, and,

The charge of powder is an ounce (which is too much). An iron wire called the pricker is put down in the hole close to the side, and small pieces of spar are dropped in, which are slightly rammed and afterwards more firmly. The whole being full, the pricker is drawn, and a wheat straw filled with gunpowder is put down in place of the pricker.

and, as well as he can, dries his mine; then inserts his charge of gunpowder, often amounting to the unnecessary quantity of an ounce, add dropping the pricker to the bottom, with its side touching the side of the mine, he begins by dropping into it some lumps of spar; and after he has filled up about an inch, begins pounding it round the pricker with his rammer and hammer; tapping gently at first, but soon beginning to ram very hard, all the while frequently turning and loosening the circular pricker.

When the hole is quite filled, he draws it, by giving some gentle strokes on the chisel that he has now passed through the loop to draw it with.

He then takes the upper joint of a wheat straw, the smallest he can get, and having stopped the fine end with clay, if it has no knot; he afterwards places the other end, cut off very bevel and sharp, between his second and third finger of the left hand, close to where the fingers join the palm, forming his hand into a kind of basin, to keep off the wind, and drawing the open end of the straw so low between the fingers that he can but just prevent it from dropping on the ground; when pouring a small quantity of gunpowder on the orifice, and tapping with his other hand on the straw below, to shake it, it speedily is filled.

This straw must be 19 inches long for a hole of 18 at least, and a little shaved away at the bottom, but not cut open of course.

Fire is given by a piece of touch-wood,

When thrust down to the powder the train is complete, and our operator lastly lights a piece of touch-wood, and places it so that when all on fire, it shall communicate to the train; after which he withdraws to a place out of the line of explosion, and waits its effect.

—which occasions less of time by its failure and danger, when too rapid.

And here in blowing a well, I found that much time was lost; for not only does the wind occasionally blow away the touch-wood before it is all inflamed, but frequently the damp extinguishes it. I also found there was danger to the workman if it went off too soon, which the wind sometimes occasions, or his companion is too slow in haling him up; and we likewise found that when they worked by the day, and we found powder, they used an immoderate quantity.

Improvements. If a cork be thrust down the hole or well pre-

To remedy these two great evils, I pursued the following plan: the first of which was suggested to me by an ingenious neighbour and both had the desired effect.

The

The first experiment I tried was upon a single block of limestone, of about two ton weight. I charged the mine with only the common charge of a musket, as at K, over which I drove a cork, as at H, leaving one inch, or thereabout, as at I, over which I rammed spar, as at G, up to the surface of the rock.

vious to stemming, so as to leave one inch of windage, less powder will be requisite.

I then made a slit in my straw train, as at L, and passed through it, as through a loop, a cut of the German ash-tree fungus; but not liking that, as endangering the loss of the priming powder, I cut the slit in the fungus, as at N, passing the straw through the slit, and cutting a small notch on one side of the straw, as at O. When it was slid down to it, being elastic, it closed there, and filled the notch.

The German fungus or aradou is steadier and more certain than touch-wood. The straw may be thrust through a hole in this fungus, —which burns slowly, but with certainty, and is not blown out by the wind.

This match burns slow but sure, and no wind can extinguish it. A great advantage, as I have frequently witnessed, in making the new and beautiful towing path on both sides the Avon, from Bristol. One hundred men lost from ten minutes to twenty and more while getting out of the way during the blowing of a mine near the spot they were levelling, and all owing to the slow burning of the touch-wood match, or the wind blowing it aside.

This German match is, I fancy, pretty well known; it is merely the fungus of the ash-tree, macerated and hammered until it becomes as flexible as a piece of buff leather, and has been called the German match, I believe, from its general use on the upper Rhine, where, by its means, habitual smokers of tobacco can light their pipes in the open air, whatever may be the weather; and as a piece which scarcely weighs four grains is sufficient to light without danger, the largest mines, while the article is by no means dear, and always safe, inextinguishable, and regular in its burning, nothing can be more useful to the practical miner.

Account of the fungus.

With respect to sand (which I see recommended in a Dublin paper of only last week, as a new discovery in stemming), it will not always succeed, especially in those great mines of the Clifton blasters, where, often 15 or 16 lb. of powder are used at a time; but I should think if stopped with a stiff clay, it would greatly encrease the resistance, especially if sufficient windage was left over the powder.

Stemming with loose sand will not succeed in mines upon a large scale. (it is thought)

The experiment I first tried, as described above, on that plan, tore to pieces, and threw four pieces of my rock to a great useful.

The author's experiments were fully successful.

great height, shaking and leaving fit for loading, a good cart load in all; while, but for a wall at hand, my Shipham miner, as usual, despising novelties, would probably have been wounded, having been with difficulty persuaded to take that cover at four yards distance.

Thus, Sir, I have stated what I take to be improvements in this valuable art, and if they afford you or your readers any gratification, I shall not regret the trouble of putting them on paper, being always, Sir,

Your grateful reader,
G. C.

III.

Description of a new Parallel Rule, exempt from lateral Deviation; invented by Mr. J. W. BOSWELL; with an Account of the Imperfections of those already made for the same Purpose.

To Mr. NICHOLSON.

DEAR SIR,

THE common parallel rule of four pieces has been long found inconvenient, on account of the lateral deviation of the moving piece, which causes a necessity of shifting the position of the whole rule frequently, when many parallel lines are to be drawn; that, besides the loss of time which it occasions, tends also to produce error in the parallelism of these lines.

For this reason it is in no respect superior to the more simple apparatus for the same purpose, formed by a triangular plane of wood or metal, moved along a common rule; and as this latter is more steady, and serves for other purposes in drawing, it is preferred by several.

Many instruments have been contrived to draw parallel lines without being subject to the imperfections here stated; but all, that I know of, are more or less deficient in correctness, from requiring an exactness in their formation hardly attainable, or from extreme tendency to have this perfection deranged when attained.

The parallel rule with crossing connectors, and two sliding joints, is subject to both the above inconveniences. The least

Inconveniences from the lateral deviation of the common parallel rule.

It is not superior to the triangular plane and rule.

Parallel rules as yet contrived to operate without side deviation are subject to inaccuracy.

The parallel rule with sliding joints is difficult to make exact, and

play

play in the slides, or deviation in the grooves in which they are to move, must alter the parallelism of the lines drawn by it; and however exact it may be at first, the natural wear attendant on its use must demonstrably produce these imperfection; to which may be added, that the nicety of workmanship which it requires, and its complicated form, must of course render it expensive.

The instrument formed by a rule supported by two small wheels fixed to the same axis, which axis is placed so as to be parallel to the edge of the rule, is liable to be imperfect, from any difference in the diameters of the two wheels, or slight inaccuracy in the position of the axis.

This rule is also very liable to slip on the paper, and is rendered incorrect in its effects by any unevenness in the surface over which it is moved.

The parallel rule, mentioned in your ninth volume, page 212, requires an exact proportion in the length of each of its parts; and as these are all of different measures, would be liable to error in the first formation, on this account; and however exactly made, would, after a little wear, soon deviate, on account of the play which this would produce in the joints; the connectors also between the two rules, passing from different extremities, and leaving long spaces beyond the points of support, would thereby occasion any play in the joints to produce a greater deviation from parallelism in the lines drawn.

The apparatus for producing parallel lines formed by the drawing board and normal square, can hardly with propriety be classed among the instruments here treated of; whatever its accuracy may be, its cumbrous form, and the time required for fastening the paper to it, render it for many purposes very inconvenient.

These considerations induced me, about the time when the account of the parallel rule, given in your ninth volume was published, to consider how a parallel rule might be constructed not liable to slide deviation, and as free as possible from the defects of the others above stated. The instrument which then occurred to me as the best calculated for this purpose, I shall now describe; and as I have often examined it since, if it possessed any material defect, it is probable it would have become manifest before this; in which case I should not have brought it forward to public notice.

becomes inaccurate from wear.

That moving upon two wheels fixed to one axis, has the same defects, and is liable to inaccuracy from the unevenness of the paper.

That formed by several unequal joints difficult to make exact, and liable to become very incorrect in wear from its long projections beyond the points of support.

The drawing board and normal square is cumbrous, and wastes time.

Description of
Mr. Roswell's
parallel rule to
prevent lateral
deviation.

My instrument for drawing parallel lines without side deviation, is formed of three rulers, laid parallel to each other, connected by two pair of moveable pieces, all of equal length, and parallel to each other; these pieces, where they meet on the middle rule, have their extremities formed into portions of toothed wheels, which lock into each other, as may be seen in the figure: the effect of these segments of wheels thus acting in each other, is, that all the lateral motion is transferred to the middle rule, while the external rules move only in an opposite and parallel direction.

The contrivance
to prevent lateral
deviation cannot
afford its accu-
racy. Its sup-
port at each end
makes it steady.
It is easily made
exact.

This instrument will not be liable to the incorrectness of those before described, for the following reasons: 1st. The toothed segments being in no way concerned in producing the parallelism of the instrument, its accuracy of parallelism cannot be at all affected by any trifling incorrectness of formation in their parts. 2nd. All the connecting pieces being of equal length, can be formed with more certain accuracy. 3d. The connecting pieces passing from the same extremities of the external rules, give them a steady support. For these reasons, in my opinion, it possesses all the steadiness and facility of formation of the common parallel rule, while it effectually prevents the side deviation, to which the latter is liable.

It might be
made with but
one pair of
toothed seg-
ments, but two
will make it
look more uni-
form.

It is not absolutely necessary to have more than one pair of the connecting pieces made with toothed segments; but as these segments are easily formed in the clock makers engine for cutting teeth in wheels, it can add little to the expence to make the two pair in this manner, as shewn in the figure, and will make the instrument look more uniform.

The middle rule
should be made
thinner than the
other to prevent
friction.

The middle rule should also be made a little thinner than the others, to prevent friction on the paper in its lateral movement when in use.

Novelty of the
instrument con-
sists in its tooth-
ed segments.

In the description of this instrument, it will be observed, that the novelty of it consists in the application of the toothed segments of the wheels to the use mentioned; which I cannot find has ever been before used for this purpose; and I think it highly probable it has not, as, besides its not being known to gentlemen, whom I have consulted on this head, most likely to be acquainted with such matters, the simplicity of the contrivance would probably have brought it into extensive use, if it had been ever known at any former period.

Reasons for sup-
posing this in-
vention to be
new.

I mention this only to shew that, before I claim the priority of invention, I have taken some pains to investigate my pretensions;

tensions; which I think is incumbent on every man to do on such occasions: for, however fair the claim may be of invention, if a thing is well known to have been before done, it at least produces an awkward sensation to the claimer; for which reason, those who accuse others of doing this, should be the more cautious, that their accusation is fair in all its parts; for oftentimes an external resemblance may subsist between two contrivances, as between my instrument and the triple parallel ruler, and yet a small addition render their effects essentially different; thus the triple parallel ruler admits of side deviation, while my parallel ruler effectually prevents it.

A small addition to an instrument sometimes renders its effects essentially different.

My motive for publishing the account of this instrument is principally because I think it a duty incumbent on every man, who has contrived any thing that may be of use to the world, to make it known as extensively as possible, which it certainly will be by appearing in your Journal.

The instrument from which the figure was drawn was made according to my directions, by Mr. Banks, instrument maker, No. 441 in the Strand, and answers the purpose perfectly well; of course any gentlemen who desire to use parallel rulers of this kind, may have them accurately made at the same place..

This instrument made by Mr. Banks, 441, Strand.

I request the favour of your permitting the insertion, at the end of this communication, of the indication of some typographical errors, made in my paper relative to the performance at sea of the ship Economy, in your last number; and which I am the more anxious to have rectified, as some of them entirely alter the sense of the passages where they occur.

Typographical errors in Mr. Boswell's last paper relative to the ship Economy.

Page 175, line 2, erase *it* before *could*; line 6, transfer the bracket to before *when* in the next line; line 8, for *is read are*; erase the comma after *is*; and transfer the bracket to after *proof*; line 9, 10, for *direction* read *directors*; page 176, line 15, for *source* read *purse*; line 21, erase *not* before *rest*; page 179, line 22, for *point* read *front*; line 37, for *forms* read *frames*; page 180, line 3, for *searping* read *starving*.

Some errors of the press are also apparent in the side notes, but I shall not trouble you by pointing them out, as they can be rectified by the meaning of the passages to which they are added. I am, Dear Sir,

Your very respectful humble servant,

J. W. BOSWELL.

Reference

Reference to the Figure, Plate V. Fig. 2.

A A, A A. The external parallel rules, BB the central rule; C D, C D the connecting pieces, D D the segments of toothed wheels in which the connecting pieces terminate, which by their action on each other prevent side déviation in A A, A A.

IV.

Letter from an ENQUIRER, on the Waste of Fish asserted to be made on the Scottish Coast. In Reply to A. L.

To Mr. NICHOLSON,

SIR,

London, Feb. 7, 1806.

Proper spirit of inquiry and publication.

Scotch fisheries.

I AGREE most cordially with your Correspondent A. L. of Aberdeen, in page 168, with regard to the accuracy of important information when communicated to the public, and that when doubts exist, it should be given with so much modesty and diffidence, as to shew that the communicator is not certain of his subject. Of the statement I made respecting those instances of wasteful negligence in some fisheries of the north of Scotland, I am not the first; the respectable author of the statistical account of the parish of Peterhead, the Rev. Dr. Moir, has asserted the same, limited to that parish that I did. In the 16th Vol. p. 550 of that work, he says, "turbot (I believe the hollybut of the London market) is now caught frequently, and in great perfection. Thirty years ago they were seldom used here, frequently cast into the dunghill, or left to waste on the sea beach, they at present sell from four pence to one shilling each, and are rising every day in price;" in the preceding page of that volume we are informed, that "the greatest part of the cods' sounds, in this parish, are permitted to remain and rot on the sea beach, or, are cast into the dunghill, though the use and value of them as an article of food and delicacy at table have been known here for many years," and yet in the following paragraph the Doctor tells his readers, "that the crews of the ships have been sent from this town to Barryhead, to preserve the sounds, tongues, and palates of the cod caught there, and the owners have always found

found a ready market for them!" for myself, Sir, before I even hinted through your Journal, at these strong affirmations, I made it my business to enquire of some friends at Aberdeen, of the truth or falsehood of such assertions; deeming it then, as that gentleman does at present, an improbable statement; under these circumstances I cannot consider the communication you did me the favour to insert, as militating either against your correspondents rule of examination, or my own habitual scepticism:—that gentleman, in recommending accuracy of statement, ought not to have forgotten it himself; he will easily see that *Aberdeen* is not mentioned by me as being at all concerned in this waste of sustenance. My little note to you has roused the attention of A. L.—Is it wandering too much into

"the fairy regions of romance,"

to hope that the subject may obtain still farther notice? and continue to do so until it be made productive of *all the advantages it is capable of*? in that case, supposing defective information in my first notice of it (and I presume A. L. will allow I had some authority for my opinion, and that he himself has not been completely accurate) my errors will be eventually at- Good effects;
inquiry and
public remark
tended with good. Your correspondent well knows, that the assertion of Dr. Johnson about the scarcity of trees in Scotland, has had the happiest effects. How far that gentleman's question about the vend at the towns I mentioned, may be answered in the affirmative or otherwise, I have not yet sufficiently informed myself; but certainly under the circumstances I conceive to be true, those markets would be preferable to such waste. The men employed may look with confidence for a speedy sale; and, if I am not very much mistaken, these towns are supplied from the Yorkshire boats, the wind therefore which brings the one set of boats, would impede, if not totally hinder the other. Far am I from wishing to throw any obstacles in the way of so excellent a plan, as that for a society for exporting white fish from Aberdeen, but where would your correspondent send them to? "*is it not to be supposed, that fishers of the places nearest to such towns could greatly undersell them?*" this question is not a greater difficulty in the scheme suggested by me of bringing the fish to Leith, Berwick, or Newcastle, than it is to that of A. L. The fact is, that by giving that question weight, competition, in every business

business would be undone; for my own part, I do not think it any objection to either of our schemes; nay, our plans considered seriously, are nearly alike. I suggested, rather I believe in the form of a query than otherwise, the propriety of bringing fish where I know a demand exists; but that gentleman opposes my suggestion with the above question, and then proposes a plan on a similar, but more comprehensive scale! Allow me merely to correct at present one more error A. L. has, *unintentionally, I am certain*, committed in the last paragraph of his vindication of the Arbroath fishers (of whose methods, and probable waste, I hope soon to obtain a *correct* account,) It is *not* the case even for the most part in *every* large fishing town, that the fishermen "retain the bodies of the crabs, and sell the large claws only. A list of some of the towns in which that custom prevails, would, doubtless oblige many of your readers. If I am correct in my opinion of A. L. all his attempts are for the spread of useful knowledge; his candour will suggest the propriety of viewing mine also in a favourable light.

I am, Sir,

Your's and A. L.'s,

Friend and Servant,

AN ENQUIRER.

Notice of a Publication of Importance intended by the Literary and Antiquarian Society of Perth.

SIR,

IT is with great pleasure I inform you that the very respectable Literary and Antiquarian Society of Perth, intend giving a selection of their valuable papers to the public; it is much to be lamented that they have delayed doing this so long, as many of their manuscripts throw a very extensive light on the antiquities of that part of the island.

I am, Sir,

Your's truly,

N. L.

To Mr. Nicholson,

Newcastle-upon-Tyne, Jan. 28, 1806.

*Letter concerning a Library established at Aberdeen. From a
TRAVELLER.*

SIR, *York Hotel, Bridge Street, Blackfriars.*

I AM extremely glad to find, that there is a subscription library established in Aberdeen. I am astonished, however, to be informed from Mr. Crombie's paper, that none of the very learned Professors in that part of the country are engaged in the undertaking! perhaps if the subscription was raised to one guinea per annum, much more good might be effected, and those gentlemen would not then scruple to join themselves; their freedom as to pecuniary motives is well known. I hope for the sake of the general diffusion of knowledge, to find myself equally mistaken with regard to Banff, Peterhead, and Inverness. The society at Aberdeen, though young, seems to be conducted with great liberality, as appears evident from their offer of assistance to the places above mentioned, or any other that may be now forming; is it too much for a friend to the spread of useful knowledge to suggest to them an extension of the benefits of their association, to those gentlemen who are members of similar societies, whilst in Aberdeen, on condition of a return of such civilities, should any of their members be where such libraries are? a rule of this kind you Sir, have mentioned with applause in a former number,

Your's, &c.

A TRAVELLER.

February 7, 1806.

V.

A Chemical and Medical Examination of the Gizzards of White Fowls compared with Gelatine, together with an Exposition of the Characteristics of the latter when oxigenated, By M. BOUVILLON LAGRANGE.*

IT has long been understood, that the gizzards of white Gizzards of poultry possesses certain medicinal qualities. The use made of fowls medically it by many physicians may justify some reliance upon the used.

* *Annales de Chimie*, Vol. LV.

virtues attributed to it; but no one, I believe, has hitherto thought of analysing this substance.

It occurred to me, that it would be useful to the art of healing, were a few chemical facts added to the knowledge already possessed of the medical uses of gizzard, particularly after reading in the "*Journal d'Economie Rurale and Domestique, ou Bibliothèque des Propriétaires ruraux, Pluviose, an 12*;" a letter, wherein is announced the success obtained by its use in agues. As this letter contains the details of the preparation, and administration of this remedy, I shall transcribe it at length.

"Amiens, 25 Frimaire."

Letter respecting it.
Recommended as a febrifuge by the French government.

"YOU mention animal gelatine as a febrifuge, I will inform you of a more simple and less expensive remedy. I know not by what fatality this great specific has been neglected, notwithstanding it was published by government forty years ago, and in spite of its efficacy, of which I have had long experience; for, of about a thousand cases, in which during that period, I have adopted its use, I can attest the cure of eight-tenths.

"I have resided at Montpellier during fifty-six years; the climate of the place and its environs is mild and salubrious; but the inhabitants along the coast are subject to agues, on account of the vicinity of the Mediterranean, and of stagnant pools. M. de St. Priest, intendant of this province, published the order of government relative to the remedy above alluded to.

Prescription.

"*Remedy.*—This remedy consists of the gizzard of fowls, dried and pulverized.

The gizzards are washed, dried, and pulverized.

"*Preparation.*—Take the gizzard of white poultry, as fowls, turkies, &c. (I never made use of those of black fowls, as pigeons, ducks, &c.) open them, and clear away the gravel they contain; having slightly washed them, let them be put on a string and hanged in the sun, or up a chimney to dry, after which they must be reduced to powder, sifted, and kept in a bottle closely corked.

"*Dose.*—The dose is about a drachm for adults, and from half a dram to a scruple for children.

The dose is one drachm taken in wine.

"*Mode of taking.*—Mix the proper quantity of the powder in a glass or half-glass of good old white wine, and let the patient

patient swallow it about half an hour before the fit comes on, or on the appearance of the precursory symptoms of the fever. This being thrice repeated, it rarely happens that the disorder returns.

Regimen.—A wholesome regimen is all that is necessary during the administration of this medicine, but the patient should carefully avoid exposure to moisture or cold, particularly in the feet."

The foregoing details lead us naturally to the following observation:

Should this substance be considered as gelatine, and possessing the same property of being a febrifuge, as stated by M. Seguin; or should we rather acknowledge it to be possessed of those particular virtues which have been attributed to it by several eminent physicians? M. Pia, an old apothecary of Paris, assured me, that full thirty or forty years ago, the powdered gizzard of poultry was recommended in all obstructions of the urinary passage, in complaints of the bladder occasioned by slimy matter, as well as in all nephritic pains.

"The efficacy of this remedy has long ago established its efficacy, use;" and the writer adds, "that during my practice in pharmacy, I have prepared large quantities of it; so much were physicians and their patients satisfied with its operation."

His method of preparing the gizzards was to choose those of young fowls, and particularly of pullets: after cleansing, rubbing, washing, and wiping them carefully, he strung them, and left them to dry on hurdles between sheets of paper, assisted by the gentle heat of a stove, and not in the sun, which, according to M. Pia, would have spoiled them.

When the gizzards were properly dried, they became friable, almost transparent, and exhibited on being broken a vitreous appearance.

The powder obtained was of a whitish grey ash-colour, yielding in the mouth a kind of mucilage, and possessing a slightly salt and bitter taste.

This powder was administered twice a day, (morning and evening) in doses of twenty-four to thirty-six grains, in a glass of the infusion of pellitory of the wall; of bearberries (*ursæ ursi*) or of lintseed sweetened with syrup.

Effects.

The efficacy of this remedy as a diuretic and aperient, was so much relied on, that the afflicted even omitted the infusion and took it in pure water with a little sugar.

Arguments in its favour.

The long experience which has been had of the salutary effects of gizzard as a febrifuge, diuretic, aperient, &c. and the publicity which the government, doubtless not upon light grounds, has given to this remedy, are authorities in its favour; and it must therefore be an acceptable labour to the physician, to furnish him with new lights upon an object so essentially interesting to humanity. This is the motive by which I have been induced to submit the following experiments to the society of medicine.

Inquiries as to its composition and use.

As gizzard has a great analogy to gelatine, I endeavoured to discover their similitude. If gelatine be really a febrifuge, gizzard should be so likewise, particularly as it contains, when fresh, a large portion of that substance; but whence does it derive its power as a diuretic, aperient, &c.? does it possess it in common with gelatine? I cannot tell. Or, have the saline parts of its composition this double property? of this also I am ignorant; for practice has not yet ascertained whether the anti-febrile quality should be ascribed to the acidulous salts rather than to the substances with which they are combined.

Experiments on recent gizzard.

A fresh gizzard presented the following phenomena.

A. The water wherein this substance had been boiled acquired a yellowish white colour, and flakes were deposited in cooling; it had a taste rather insipid than sweet.

It reddened the tincture of turnsol.

B. Lime water, and water of barytes produced in this liquor an abundant precipitate, partially soluble by nitric and muriatic acids.

C. Ammonia caused a less degree of precipitation.

D. Oxygenated muriatic acid separated with flakes from the liquor.

E. Caustic potash, either solid or liquid, acted upon gizzard in the same manner as upon muscular flesh.

When ground together, ammonia was disengaged from the gizzard; it became soft, of a reddish colour, and soluble in water. If this liquor be evaporated, it will deposit fibres in cooling. Alcohol, by destroying the potash, separated a flaky substance, soluble in water.

This

This aqueous solution gave a precipitate on the addition of lime-water, or muriate of lime or of barytes, as well as of some acids. The precipitate obtained by lime-water may be redissolved by the addition of more water, which proves that the mixture had not become truly saponaceous, but that the potash had merely dissolved the animal matter.

F. The action of certain metallic solutions on the liquor of fresh gizzard was more or less perceptible, according to the facility with which the metal communicated its oxygen to the animal matter.

Nitrates of mercury and of silver, for example, were decomposed, but the precipitates obtained by the action of these salts upon gelatine and the extract, quickly turned black, particularly that of mercury, and they were no longer soluble in nitric acid. The oxides had, therefore communicated a part of their oxygen to the gelatine and the extractive matter, which were thus united to the mercury, now approaching a metalline state.

Oxygenated muriate of mercury was not decomposed in this manner. The circumstances, in fact, were no longer alike: the excess of oxygen which it contains sufficing to oxygenate the two substances. Here the precipitate was very little coloured, and the metallic salt was only restored to the state of mild mercurial muriate.

Some other metallic solutions produced in the liquor of fresh gizzard only gelatinous flakes; such are the acetate of lead, and the sulphate of copper and iron.

G. Aqueous tincture of nutgall changed the liquor into a kind of jelly.

I have thought these experiments sufficient for demonstrating the nature of those substances which were capable of solution in water; yet as gizzard is not administered in its fresh state, but undergoes a process which might cause a variation in the foregoing results, I again examined it in this latter point of view.

In drying the gizzard, I followed the prescription already cited of M. Pia, and obtained a substance exactly answering his description.

A. Reduced to powder, its taste was insipid, yet partaking strongly of an animal flavour; its colour was a whitish grey.

Experiments on
dried gizzard.

B. The aqueous decoction took a light yellow tint, and smelled like chicken broth.

It reddened the tincture of turnsol.

C. Lime-water and water of barytes caused the same kind of precipitate as in the decoction of fresh gizzard.

D. Oxalate of ammonia proved the presence of lime.

E. Oxigenated muriatic acid separated white flakes.

F. Nitric acid had a violent effect upon the dry gizzard; at a mild temperature it dissolved it completely.

Nitric acid at eighteen degrees excited a slight effervescence, and by gradually increasing its temperature, a separation was perceived of azotic gas, then of nitrous gas, and of carbonic acid gas.

The liquor left in the retort was evaporated, in the expectation of obtaining crystals; but on cooling, none appeared. The evaporation was then continued, the result of which was a yellowish glutinous matter, tenacious, and of an excessively bitter and acrid taste.

Water imbibed the acid, and presented all the characters of the decoction of apples.

G. Metallic solutions presented nothing particular, as in the experiments upon fresh gizzard, except that antimonial tartrate of potash was decomposed, forming in the decoction a white precipitate.

H. Aqueous infusion of nut-gall produced a less copious precipitate in this experiment, than it had with that upon fresh gizzard.

I. Dry and friable gizzard was digested in alcohol; but the liquor was scarcely coloured, even with the assistance of caloric.

This alcoholic tincture reddened that of turnsol, and gave precipitates with lime-water and water of barytes, as also with nitrate of silver; a proof that the alcohol has dissolved only the saline particles.

L. The incineration of gizzard left a residue of a saline and alkaline taste. Paper tinged by curcuma became of a deep brown.

This residue was partly soluble in water. The liquor contained sulphate, muriate, and carbonate of potash.

The part not soluble, on being submitted to the action of muriatic acid, discovered carbonate of lime, phosphate of lime, and a small portion of iron.

Hence

Hence it results, that the greater part of the salts contained in gizzard, is the acid phosphate of lime; the presence of muriate and sulphate of potash is also observable.

These salts are not only united with gelatine, but also with a small quantity of extractive matter. It should seem that the latter substance, and perhaps the gelatine, is oxygenated by the desiccation of the gizzard; for in this state they are less soluble in water.

Wishing to ascertain the difference between pure gelatine, and that which had been oxygenated, I made experiments upon the former, of which the following is the result.

Pure gelatine acquires different properties, according to the means employed in its oxygenation. Experiments on gelatin,

Of the metallic oxides, some freely communicate their oxygen to gelatine, as the oxide of red-lead, and the red oxide of mercury; but the gelatine was combined with a part of the oxide, and could not again be separated completely from it. In treating gelatine with the red oxide of mercury, a part of the oxide was restored to its metallic state, and the remainder assumed a reddish brown colour. with metallic oxides.

Superoxigenated muriate of potash heated with gelatine, caused no alteration in its nature. And other means of oxygenation.

Oxygen gas combined with it but slowly, and in small quantity. After being for a considerable time submitted to the action of this gas, the gelatine only suffered a change of colour; it became whitish, but its characteristics are still the same.

Oxygenated muriatic acid presented the following phenomena.

On pouring oxygenated muriatic acid gas into dissolved gelatine, a whitish thick scum appeared on the surface, of a moderate thickness, the under side of which gradually changed colour, and became milky. The white filaments which swam in the liquor, together with the scum which floated on the surface, were separated by filtering, and washed in cold and warm water till the water ceased to redden tincture of turnsol. The substance thus prepared presented the following characteristics:

1. It was capable of extension equally with gluten, and was of a white colour. Properties of oxygenated gelatin.

2. It

2. It was very light, and swam upon water.
3. When well washed, it retained little or no flavour,
4. Left exposed to the air, it dried, and fell to dust.
5. It did not redden the tincture of turnsol.
6. It was scarcely at all soluble in warm water. On boiling it a length of time, in a sufficient quantity of water, it was reduced to an infinite number of particles, so minute as to be hardly perceptible; but as the heat was lowered, they reunited in a mass as before the boiling.
7. Heated nitric and acetic acids dissolved this substance; but it was precipitated in its original form, by refrigeration.
8. Trituration with caustic potash produced a separation of ammoniac.

It differs from albumen.

The gelatin of gizzard is probably oxygenated.

This matter, it will be perceived, is neither gelatine nor albumen, since its properties are wholly different.

It appears probable, that the gelatine in gizzard acquires by drying, properties analogous to those above described; which, with the changes observed in the extractive matter already mentioned, would certainly render dried gizzard less soluble in water.

We have no means of ascertaining, for want of a proper object of comparison, whether this difference be essential to the efficacy of gizzard; and I know not if fresh gizzard has ever been adopted in medical practice. I could only wish to ascertain if its febrifuge quality exist in the oxygenated gelatine, in the extractive matter, or in the acid salt. Indeed, on comparing the quantity of gelatine administered to patients, according to M. Seguin, with the dose of powdered gizzard, above-described, a great difference will be observed; and yet according to those who have made use of it, a small dose of powdered gizzard is sufficient to check the fever.

The comparison which I have made of gelatine with gizzard is sufficient to establish a material distinction between them.

Experiments on gelatine.

Pure gelatine possesses a weak insipid flavour; does not redden tincture of turnsol; is mucous and gluey between the fingers; assumes in the fire a concrete, solid, and transparent appearance; and is soluble in boiling water.

Solution of barytes or of lime mixed with that of gelatine, causes a precipitation of phosphate of lime.

Sulphates

Sulphates of copper or tin, and acetate of lead, experience ^{Experiments on} no decomposition. ^{gelatine.}

Nitrates of mercury and silver are decomposed, but the precipitates are much less copious than those produced with the decoction of gizzards.

Solution of tartrite of antimony only thickened the liquor.

Alcohol likewise has but little power over gelatine. The precipitates obtained by means of the water of lime or of barytes, as well as that by nitrate of silver, are scarcely perceptible.

The decoction of fresh gizzard when suitably evaporated, leaves a coloured gelatinous matter, soluble in water, which reddens tincture of turnsol; gives copious precipitates with lime-water and water of barytes; decomposes sulphates of iron and copper, acetate of lead, muriate of tin, tartrite of antimonial potash, and nitrates of mercury and silver; the precipitates resulting from these decompositions are generally too considerable to be attributed solely to the gelatine.

Dried and powdered gizzard possesses characteristics still more distinct from those of pure gelatine, whence I conclude that the latter substance has a different operation.

I leave practitioners to decide on the advantages which the medical art may derive from gizzard; it is for them to decide whether much confidence is to be placed in the notice inserted in the *Journal d'Economique*. And if it shall appear that the medical use of this material has been attended with success; it will perhaps be proper to attend particularly to other substances which have not hitherto been supposed to possess any febrifuge virtue; such as the salts with excess of acid, the oxygenated extractive and even oxygenated gelatine.

VI.

*On Pirite found in France by M. Cocq, Commissary of Gunpowder and Saltpetre Works at Clermont-Ferrant, with an Analysis of this Substance. By J. J. DRAFFIER, Teacher of Chemistry at the Polytechnic School.**

Crystals of pirite found in the district of Puy de Dome,

—and on the way to Menat.

IVI. COCQ found the crystals of pirite, in a porous grey porphyry, with a base of feldspath, and containing crystals of quartz, forming a part of that chain of primitive mountains which support the volcanoes of the district of Puy-de-Dome. These crystals of pirite separate from the rock, and leave in the porphyry an impression perfectly smooth.

He also found at the village of St. Avit, and in the vicinity of Pont-Gibaud, a substance which appeared to be pirite; in both situations it was so indeterminate as to render it impossible to pronounce exactly on its nature. But in returning to Menat, at twelve leagues to the north of Clermont, he perceived the granites resuming the same appearance of those which he had observed near Saint Avit and Pont-Gibaud, sometimes the colour of the feldspath inclined to purple, and oftentimes this substance appearing alone in the mass of the granite, exhibited a beautiful purple.

The grey porous granite appeared again at intervals, with the appearance of the crystals observed in the same rock near St. Avit and Pont-Gibaud; at last, after a great many searches, he found the pirite well defined, and assuming a character much more determinate than that of Scheenberg.

Its Physical Characteristics.

Each crystal is a prism of twelve faces, of a blackish or greenish brown, with a smooth surface.

Its colour is a greenish or blackish brown. Its form is a regular hexedral prism, of which all the lateral edges are truncated, which constitutes it a prism of twelve faces. Sometimes the prism has also a small face at each of the angles of its base, which has not hitherto been remarked in the pirite of Saxony.

The surface of the crystals is smooth, and a little brilliant: in its interior, the pirite is dull, containing at times some particles of mica.

* Journal des Mines, Vol. XVII. p. 307.

Its fracture is unequal, with a fine grain, approaching to a splintery fracture.

It admits of being scraped by a knife, and yields a dust of a bright grey colour; it is tender, and does not adhere to the tongue, though it is a little unctuous to the touch.

Besides the size of its crystals, their faces, the substances to which they are found attached, added to the characters described, establish the identity of this mineral with the pirite of Saxony.

The crystals found in Auvergne are more perfect than those of Scheenberg; they exhibit no alteration, and the purity of their form removes all doubt of there being any necessity to class this substance as a new species.

Analysis by M. Drappier,

The pirite of France, separated carefully from its bed, and reduced to a fine powder, is attacked and discoloured by muriatic acid.

This acid dissolves the oxide of iron, the colouring principle, and a portion of the alumine: but as it leaves a considerable residue, on which it appears to have no action, M. Drappier thought the method of analysis should be changed:

he then took 100 parts of this substance, and kept it at a red heat in a crucible of platina for half an hour; after it was cooled, there was a loss of seven parts.

The remaining 93 parts were heated in the crucible for three quarters of an hour, with three times their weight of caustic potash, purified by alcohol. The fused mass, detached from the crucible by distilled water, dissolved entirely in muriatic acid.

The solution evaporated almost to dryness, and then diluted with a fresh quantity of water, let fall a white precipitate, having all the characters of filix. This precipitate washed carefully and well dried, formed 0.46 of the substance submitted to experiment.

The remainder of the muriatic solution was decomposed by caustic potash. It immediately formed a precipitate, which soon dissolved again in the excess of alkali, with the exception of $2\frac{1}{2}$ parts of oxide of iron.

The alkaline solution saturated by an acid, deposited 42 parts of an earth, which had all the properties of alumine. All these precipitates, before they were weighed, were washed carefully, and heated to redness in a crucible of platina.

Analysis

Analysis of the Pirites of France compared with that of Saxony

	Pirite of France.	Pirite of Saxony analysed by Klaproth.
Analysis tabu- lated.	Silex - - - - 46,00	29,50
	Alumine - - - 42,00	63,75
	Oxide of iron - 2,50	6,75
	Loss by calcination 7,00	
	Loss - - - - 2,50	
	100,00	100,00

Remarks on the difference of the pirite of France from that of Saxony.

M. Drappier thinks, on comparing his analysis with that made by M. Klaproth, that it may be concluded, supposing there was no error in either analysis, that either the pirite of France is not the same substance as that of Saxony, or that minerals having the same external characters, and especially the same form, may vary both in their chemical properties, and in the proportions of their constituent principles. M. Klaproth says that acids have no action on the pirite of Saxony, that he found much difficulty in operating on it by potash, and that, in order to separate its parts, he was obliged to treat it twice with this alkali. The same chemist appears not to have found any water in this substance. This difference, it is true, may be explained, if it is considered that the pirite of Saxony contains more alumine, and that it adheres to the tongue, while that of France has not this property, probably on account of the water which it contains.

VII.

Experiments, shewing, contrary to the Assertions of Morichini, that the Enamel of Teeth does not contain Fluoric Acid. In a Letter from WM. BRANDE, Esq.

To Mr. NICHOLSON.

SIR,

Gay Lussac on
fluoric acid in
animal sub-
stances.

HAVING seen in one of the last numbers of the *Annales de Chimie*, an article entitled, "Lettre de Monsieur Gay-Lussac a Monsieur Berthollet, sur le presence de l'acide fluorique dans les substances animales," &c. I was surprised to find that a chemist at Rome, of the name of Morichini, had discovered fluoric

fluoric acid united to lime in the enamel of human teeth. The extraordinary results of these researches, induced me to repeat them; but before I mention the experiments from which I have drawn conclusions different from those of the above-mentioned chemist, it may perhaps be proper to quote that part of Gay-Lussac's letter which relates to the present subject:—and said by Morichini to exist in the enamel of teeth.

"M. Morichini having detached some of the enamel from human teeth, supposed that it might bear some resemblance in its composition to the enamel of the fossil teeth of an elephant, in which, on a former occasion, he had detached fluoric acid; he therefore subjected it to analysis, and perceived, to his great satisfaction, that it contained a large proportion of fluoric acid. Quotation to that effect.

To render these experiments more conclusive, he submitted portions of the two species of enamel, viz. that of the fossil, and human teeth, and likewise fluat of lime, to the action of sulphuric acid, and found that the last of these three substances yielded fluoric acid in the greatest abundance, and that the enamel of fossil teeth yielded somewhat more than that of human teeth; but Morichini remarks, that this difference is merely owing to the presence of animal matter in the two kinds of enamel, and that the disengagement of the acid from the fluat may be retarded, by adding a little gelatine to that substance, after it has been calcined, and then drying the compound. He moreover observes that the vapours which sulphuric acid disengaged from any of these three substances, had the property of acting on glass, of depositing a siliceous film on water, and other properties, which it is scarcely necessary to mention. Morichini says that the enamel of recent teeth afforded fluoric acid as well as that of fossil teeth; and that the sulphuric acid disengaged vapours that corrode glass, &c.

According to Morichini's experiments, one hundred parts of the enamel of human teeth contain 30 parts of animal substance, and 22 parts of fluat and phosphate of lime, with some magnesia, alumine, and carbonic acid. He has not yet been able to separate the fluoric and phosphoric acids from each other, but thinks that the proportion of the latter must be extremely minute. M. Morichini has also observed that the enamel of the fossil teeth of the elephant differs from that of human teeth, in containing a smaller proportion of animal substance and phosphoric acid; but he thinks that the phosphoric acid which is found in the enamel of human teeth may have been derived from Component parts of enamel of teeth according to Morichini.

from a portion of the bony part from which the enamel is separated with great difficulty. But the most interesting and unexpected result is, that fluoric acid exists in animal substances: a discovery of the greatest importance. These experiments oppose the present opinion concerning the composition of enamel, for Mr. Hatchett in his analysis of this substance has only detected phosphate of lime.

Morichini professes to have proved my facts by repeated experiments.

The result of Mr. Hatchett's experiments, together with those which were subsequently published by Mr. Jöffe, in the *Annales de Chimie*, Tom XLIII. rendered it necessary for M. Morichini to submit his opinion to accurate investigation, and after having made a numerous series of experiments on the subject, he observes, that he cannot entertain a doubt, that the enamel of human teeth consists chiefly of fluat of lime.

General remarks by Gay-Lussac.

After some observations on the composition of ivory, M. Gay-Lussac concludes this part of his letter, by observing that there is an immense field laid open in that part of chemistry which relates to animal substances, if it were merely to search for fluoric acid. Morichini has undertaken an investigation of the subject: but so much remains to be done, that the exertions of many chemists will be requisite."

The author's experiments shew the contrary.

I shall now relate some experiments, which will shew that fluoric acid does not exist in the enamel of human teeth, but that this substance consists chiefly of phosphate of lime, as originally stated by Mr. Hatchett.*

Enamel of human teeth was ignited, pulverized, and subjected to sulphuric acid. The fumes did not corrode glass.

One hundred grains of the enamel of human teeth, detached from what is usually termed the bony part, but which appears to consist of a substance of the nature of ivory, were kept for a few minutes in a red heat, and then pulverized. The enamel, thus reduced to powder, was put into a platina crucible, in which a piece of a glass rod was placed horizontally in such a manner as to be about an inch and a half above the enamel. Half an ounce of sulphuric acid was then added, and the crucible being covered with a clean plate of glass, the heat of a lamp was applied, and distillation carried on for half an hour. During the process, white suffocating fumes were extricated; but on removing the glass which closed the top of the crucible, neither this, nor the rod below it were in the least acted upon; which certainly would have happened, had

* Vide Phil. Trans. 1799, p. 328.

any fluoric acid been present. Finding this, therefore, to be the case, I proceeded as follows:—Fifty grains of the same enamel were introduced into a small glass retort, and a little sulphuric acid being added, distillation was carried on nearly to dryness, but in such a manner, that the gaseous products might be received over mercury. A small quantity of sulphuric acid gas was disengaged, and what remained in the retort, consisted, as far as I could ascertain, of a mixture of sulphate of lime, phosphoric acid, and a small portion of sulphur, arising from a decomposition of a small part of the sulphuric acid by the animal matter, existing in the enamel.

neither was there any extrication of fluoric acid by adding sulph. acid to enamel and distilling over mercury.

I have the honour to be,

Sir,

Your most obedient servant,

WILLIAM BRANDE.

Arlington Street,
Feb. 15, 1806.

VIII.

*A Memoir on taking the Levels of the whole Surface of France
By P. S. GIRARD, Chief Engineer of Bridges and Highways, &c.**

IF the surface of the earth were formed by the revolution of a curve round its axis, it would be sufficient, in order to determine the respective positions of different points upon it, to measure their distances from the intersection of that surface made by the plane of the equator and any particular or assumed meridian.

Thus geographers, considering the earth as perfectly spherical, have determined the position of any given place by the conjunction of two co-ordinates, one of which is the arc of the meridian, comprised between the place and the equator, and the other an arc of the circle parallel to the equator, comprised between the place and any assigned meridian.

The method by which geographers determine the position of a place,

* Journal des Mines, Vol. XVII. p. 297.

As these two co-ordinates intersect each other at right angles, it is apparent that the method of geographers, for determining the position of any place on the earth, is the same as that by which the position of a point on a plane is commonly determined.

is not exact, on account of the inequality of the earth's surface.

But this process, which would completely answer the views of geographers, if the terrestrial sphere were regular, ceases to be exact when the irregularities and protuberances are considered, with which the surface of this spheroid is covered.

This true position of a place is in a line perpendicular to that assigned by geographers.

The position of any place depends in reality, according to this hypothesis, on a third co-ordinate, which is supposed to be drawn perpendicular to the point of intersection of the two others.

This third co-ordinate ought to be taken vertically over the place of which the position is to be determined, and its measure reckoned from the place itself to its arrival at an imaginary surface, produced by the revolution of a known curve round the axis of the earth.

The level of the sea affords a spherical surface, from whence to measure those perpendiculars.

But it is known, that if our globe were surrounded by a fluid mass, all other force being supposed to be absent but that of terrestrial gravity, the surface of this fluid mass would be that of a spherical solid, of which the mean surface of the sea, in its actual state, represents a part: It appears then convenient to choose, for the third co-ordinate here mentioned, that portion of a vertical line passing through any place, which is comprised between that place and the mean surface of the sea, supposed to penetrate the globe and to be extended beneath the continent.

This is the best, though not the only method:

We have said that the choice of this line would be convenient; because, in reality, the position of a point on the terrestrial surface may be determined by adopting any other system of co-ordinates; for example, by fixing the position of this point, by three planes mutually intersecting at right angles; but, besides the advantage of greater simplicity in the expression of the circular co-ordinates, they have moreover, that of being generally adopted; for the geographical charts, hitherto prepared, may be considered as the projection of the continents and islands on the mean surface of the sea; so that there only remains, in order to render geography perfect, to add to the latitude and longitude of all the places on the earth, the

But it is the most simple, and is besides generally adopted.

The true position of a place determined by annexing its

vertical

vertical height which they are elevated above the surface of the ocean.

The object of this memoir is to indicate the means of determining this vertical height, by their particular application to the territory of France.

It is evident, that all the operations necessary for this determination, may be reduced to a series of levels made in determinate directions.

Nature itself has pointed out these directions, by the lines of greatest declivity, which the large rivers, and those which flow into them, form on the surface of the earth.

Thus, France being divided into five principal basins, by the Rhine, the Seine, the Loire, the Gironde, and the Rhone—the levels of the course of these rivers, from their sources, or from their entrance into France, to their terminations in the ocean, would form the first basis of the work proposed to be undertaken.

After having ascertained this first basis of the general operation, the levels of the streams by which the great rivers are supplied, should be next taken, and these streams should be considered without any regard to those of the third order, by which they are themselves maintained.

At the same time, the levels of the rivers of the second rank, which fall into the two seas, should be taken; such as the Escaut, the Somme, the Orne, the Vilaine, the Charente, the Adour, the Herault, &c.

The declivities of the beds of the secondary rivers being known, those of the rivers of the third, fourth, and fifth orders, &c. should be determined successively, according to special instructions which should be given for this purpose.

By thus classing the operations relative to the general levels of France, and by arranging their results in order, as they were obtained, all the data would be soon collected, which were necessary for tracing the elevation of its territory on a geographical chart already prepared.

This tracing of the elevations would be effected, by joining all the points on one level by the same line.

These lines of levels might be supposed to be elevated perpendicularly, one above the other, by a determinate space, conformable to the scale of the chart on which they were traced.

above the sea to its latitude and longitude.

Management proper for ascertaining these positions, shown in the instance of France.

The levels of the five great rivers of France would form the basis of this operation for that country.

The levels of the streams which supply those great rivers should be next taken.

The levels of the rivers of the second rank taken at the same time as those of the first.

The levels of rivers of the third, fourth, and fifth order, taken.

Elevation of the surface of France to be expressed in the rough, by joining all the points on one level by the same line.

It is evident; that these lines would represent the borders of the coasts of the sea, if it was supposed that its mean level should be elevated successively to the same heights which they represented.

M. Triel prepared the sketch of a map on this plan.

It was according to this idea that M. Dupain Triel prepared a physical chart, mentioned by M.^o Lacroix, member of the National Institute, in his introduction to Pinkerton's Geography; a chart which, from the defect of materials necessary for its construction, presented only the sketch of a work, the extent of which would require for its perfection an union of means, which could not be at the disposal of any particular individual.*

The order has been pointed out in which this work ought to be executed, and we shall now examine how it should be performed.

The sources of the great rivers should be divided into portions, and the levels of each taken by observers at the same time.

The bed of each of the great rivers must be divided into a certain number of portions, and each portion should be levelled by observers, who should operate at the same time.

These observers should place accounts of their operations at each extremity of the portions of the basins with which they were charged; and as the levels of the secondary rivers should be connected with those of the principal rivers, it would be necessary also to place accounts of the operations at the mouth of each of the influent streams.

The levels should be taken on the banks of the rivers, without any regard to the surface of the water. If it were thought useful to determine the declivity of this surface, it would be easy to ascertain it, by levels taken at the same time with the others, at certain distances from each other.

The results to be collected into a general system;

When the different observers have completed their respective observations, the results must be collected, to form the series of levels of one of the beds. And in the same manner the levels of all the rest should be obtained.

After this, a general system should be formed from those particular levels, by connecting together the different beds, by operations directed from one to the other, according to those lines which would afford the greatest facility.

* Compare this with Mr. Churchill's plan, at p. 224 of our Xth. Vol.—T.

There only remains to determine to what agents Government should entrust the performance of the general levelling of France, in order to have it executed with the greatest exactness, speed, and economy.

The engineers of bridges and highways, already placed in the different departments, where this operation should be performed, are evidently the only persons to whom it could be confided, so as to fulfil these three conditions.

The engineers of bridges and highways would be the most proper persons to employ in this work.

In fact, the execution of all projects relative to the establishment of communications by land or by water, require, that the elevation in relief of the country, through which the works should be carried, should be known. The theory and practice of levelling form an essential part of the instruction given to the engineers of bridges and highways; and greater reliance may be placed on the exactness of the results which they might furnish, because the use of the instruments necessary to this operation, is more familiar to them.

On the other hand, there are none of those engineers who could not dedicate some days of the summer to taking the levels of that portion of such great rivers, or streams, as shall traverse his district; and as it is easy to take the levels of four or five kilometres (about three English miles) each day, especially when the line to be levelled is previously determined by the direction of the river or current of water, it is certain, that the engineers of the bridges and highways might collect, in a very short period, very minutely detailed materials for a physical chart of France.

Lastly, these materials would be collected by them with the least possible expence, because Government would neither have to support the cost of extraordinary journies, nor the purchase of instruments, as the engineers are already, by the very nature of their employments, dispersed over the several districts where it would be necessary to operate, and are, at the same time, provided with the different instruments required for this purpose.

They could perform it without expence to the nation.

It may also be added, that the taking the general levels of France appears to be, with the more propriety, a work that should be performed by the engineers of bridges and highways, as they would be the first to profit by this operation in putting their projects into execution.

It would tend much to their own benefit to have it effected.

Suppose then that the engineers of bridges and highways were charged with the performance of this work, let us consider how, after some years, the exactness of the result, which they had collected, could be sufficiently ascertained.

Let us take, for example, the bed of the Loire, whose course is of great extent.

The chief engineers of the departments of the Upper Loire, of the Loire, of the Saone and Loire, of the Nievre, of the Loiret, of the Loire and Cher, of the Indre and Loire, of the Mayenne and Loire, and of the Lower Loire, would be ordered to furnish, during the year, the levels of that part of the course of the Loire which traversed their respective departments.

According to the new organization of the service of bridges and highways, these nine departments require twenty-two engineers, in the district of each of whom would be found a portion of the work to be performed.

The total extent of the Loire is about ninety mariameters (about 550 miles), which being divided among twenty-two observers, would give to each of them little more than forty kilometers (about twenty-five miles) of levels to execute.

Levels of the Loire might be taken by the 22 engineers in that district, in one season.

There is reason to believe, from experience, that the twenty-two engineers employed on the course of the river, would finish, in less than one season, the levels of the whole river.

The same thing may be affirmed of the engineers placed in the departments traversed by the Rhine, the Seine, the Gironde, and the Rhone. It appears then beyond a doubt, that, at the end of the first year, the chief part of this physical chart could be completed, to which the farther details might be afterwards added.

Levels of France, when taken, might be afterwards verified by the newly appointed engineers.

Whatever care may be bestowed in taking levels, their verification is always an useful operation. That of the general levels of France might be made as often, and in whatever circumstances it should be judged necessary. It would be sufficient for this purpose, to direct the newly appointed engineers to repeat, in the departments to which they might be sent, the observations of their predecessors; which, besides the advantage of confirming or correcting the results already obtained, would give an opportunity to the new engineers of acquiring, in person, a knowledge of the elevation of their respective districts in relief.

The

The facility and promptitude with which the engineers of bridges and highways might execute this work, will be apparent, if it be recollected that, at the time when the major part of the great roads in France were formed, and when a general system of internal communications was desired to be established, M. de Trudaine, assisted by M. Perronnet, caused plans to be taken of all the principal roads, from their commencement to the frontiers. There was joined to the plan of the road properly so called, that of the country bordering on it, to the distance of three or four hundred yards at each side; a work which evidently required more time than simply taking the levels of a determined line, such as we propose; and yet the engineers of bridges and highways, or their pupils, employed in taking those itinerary plans, completed from five to six leagues of them each month.

The general utility of the operation, of which a sketch is here given, will sooner or later determine some of the nations of Europe to undertake it. France, on whose territory has been lately executed some of the finest geological operations which were ever performed, and where, for the first time, a system of universal measure has been established on an inviolable basis, seems to be particularly called on, to give, on this occasion, the first example of a work, which, by completing the natural geography of countries, will furnish new facts to geology, and to those different parts of natural history which depend on it.

IX.

Observations and Experiments on the Composition of Water, and other Elementary Doctrines. By H. B. K.

To Mr. NICHOLSON.

SIR,

AS two papers have appeared in your Journal, both of which militate against the result of my experiment, and as Mr. Accum has been concerned in one of them, I therefore think it incumbent on me to answer them.

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I was

The gases obtained from water by galvanism, smelled of nitrous gas after explosion, and gave nitre with potash.

On Pacchioni and Riffant's experiments.

I was glad to see my experiments in your Journal, as it has so extensive a circulation. I shall now give you the analytical part to confirm my former experiments; as by them I had, I hope, given strong evidence, that acids are necessary in forming gases. Having collected a great quantity of the gases produced by the galvanic pile, I introduced them into a strong glass tube, closed at one end, the other end I afterwards closed, having previously introduced to the gases a small quantity of a solution of potash: through this tube the electric spark was made to pass, it having small openings to admit the wires of communication. Upon their combustion, the smell of the nitrous acid vapour appeared, both from its colour and smell; and the tube being moved up and down, so as to allow the vapour and the solution of potash to come in contact; the solution being examined some short time after, it gave evident and unequivocal signs of the nitrate of potash.

I see in your Journal, Mr. Riffant's experiments, in answer to Mr. Pacchiani's paper. Indeed, in reading Mr. P.'s experiments, nothing could appear more vague and wild than that water, by having oxygen, the supposed acidifying principle, taken from it, should become a strong mineral acid. Mr. Riffant's second experiment directly contradicts my experiment, on the supposition that water is a compound body; but if examined upon my supposition, that the acids are necessary in forming the gases, and that the water is only necessary in forming the water of composition, I hope I shall be able to prove that his experiment confirms my opinion. There were very little of gases formed by this experiment, and the wires were very much calcined: now this calcination was from the acid, or acids, I proved by repeating the very same experiments; but only instead of distilled water, I used a solution of potash, and instead of the wires being calcined, they were not sensibly acted upon, and the potash became nitrated.—Now, Mr. Nicholson, I (seriously and ardently) call upon your numerous readers to perform this experiment, which I think must be decisive.

Against the doctrine of the composition of water: it is urged that the gases obtained in galvanism vary from different causes.

I can but smile at the French chemists, in making the proportion of the gases so exactly to tally with their opinion of the composition of water; but I have in my experiments found very different results; the kind of gases depending a good deal upon the wires used, the different metals, their length, and

the

the different liquors between the plates of the pile; all of which had a sensible effect upon the gases, both upon the quantity produced, and their kind; the calcinable wires when long producing the most inflammable kinds, and the less calcinable metals the more of the oxygen kind, and the longer the wires the more in volume were the gases.

Mr. Northmore, in your Journal, endeavours to prove the formation of the nitric acid from the compression of gases.—Remarks on Mr. Northmore's experiments. Upon investigation, his experiments will, I think, be found very vague and inconclusive: that gases from active compression will produce both heat and water, has been long known. The first experiment was in condensing hydrogen, oxygen and nitrogen gases, two pints of each. He says they produced "white floating vapours, probably the gaseous oxide;" but in experiment the seventh, he observes, "the hydrogen produced white clouds at first, *quære ammonia*." So without any chemical examination of these white clouds, they are at first supposed to be the gaseous oxide, and afterwards ammonia, just according as his theory dictates to him. In the fifth experiment, he says, "and the result was only a smell of gaseous oxide of nitrogen, a few yellowish fumes." Here then the gaseous oxide produces a yellowish colour, though in the first experiment it was a white colour.

The acid produced was, from the same vague opinion, supposed to be the nitric; but this he endeavours to examine in the next experiment; first by a good test, in exposing it to lime water; and he says, "Some yellow particles were seen floating upon the lime water; these particles probably arose from the resinous substance used in fastening on the cap of the receiver being dissolved by the nitrous gas formed during condensation. Here then was the lime water affected. I say with confidence, these flocculi in the lime water were from the carbonic acid produced, and why they appeared yellow was from their being seen through the gases, being clouded with an orange colour, which, as he observes, they put on when they were condensed.

That acids are necessary in forming oxygen gas, I hope appears very clear from my experiments; therefore when it forms combustion with inflammable bodies, it is rational to suppose that an acid will appear upon its decomposition. If the combustion is active, as in the French experiments in condensing oxygen

Remarks on
Mr. North-
more's experi-
ments.

oxygen and hydrogen gases, the heat produced is so active as to make an explosion of the gases; but if a slow combustion, it will leave the oxygenized acid in a gaseous state, as carbonic acid gas, which, I suppose, was the case in Mr. Northmore's experiments. His next experiment of examining the acid: He compressed the gases upon two scruples of the solution of potash; he says, "there was scarce enough acidity to tinge the edge of the test paper; of course, I could not effect the formation of the nitrate of potash." But always to assign some reason for the failure, he says, "This quantity (of gases) was hardly sufficient for the receiver's capacity;" but there was the same quantity in this experiment as in the others; nay, in the next experiment (the sixth) there was identically the *same quantity and in the same proportions*; and in this fifth experiment, he found so little acid, as he says, "Scarce enough acidity to tinge the edge of the test paper; of course I could not effect the formation of the nitrate of potash." Now upon the supposition that the carbonic acid was formed, it would unite with the potash, and therefore the mixture would be less saturated with it: But if the acid was so strong as he speaks of in the sixth experiment, from the very same process, as he says, "Which moisture was strongly acid to the taste, coloured litmus, and when very much diluted with water, acted upon silver." Now if Mr. Northmore will consult the writings of chemists, (Dr. Black's lectures, for instance); in Vol. II. the doctor says, "that the nitric acid requires a little water to reduce it to the strength of aqua fortis; in order to act upon silver, therefore, in this experiment, the acid must have been in the concentrated state of the nitric acid, as it required water to be diluted to make it act upon silver; but probably Mr. N. does not know that water impregnated with hydrogen gas will colour silver; which I suppose to have been the case here.

This reasoning must appear to be most extraordinary: this vast quantity of nitric acid produced was even to penetrate into the cap of the receiver; but very unfortunately for this supposition, chemists are of opinion that acids will not dissolve resins. Mr. Hatchett has promoted their operation upon each other by using the strong nitric acid; but this was a difficult and tedious process, not during the transitory action of a little time, by compression; and where the resin was so condensed;

sealed; so that the acid could not get to act upon it, being placed within the cap of the receiver; therefore the quantity of weak acid formed in Mr. N.'s experiment could not rationally be supposed to have penetrated to it, even if it was in a high concentrated state; but it must have been much diluted with water, as there was water also produced in this experiment: Also, if it was in this high concentrated state, and in that abundance as to enter into all the crevices, it would easily have been detected, and his fifth experiment was for this purpose, but it failed; he could find no nitrous acid.

Remarks on
Mr. North-
more's experi-
ments.

In experiment the seventh, he supposed he had formed ammonia, and he says in this very experiment, "Some vapour was generated, which was, as usual, strongly acid." How comes it that this acid, which was supposed to find out the resin, so perfectly concealed, could not find out the ammonia, which was formed along with it in the process, and so universally dispersed as to form white clouds.

The third experiment: "Two pints of carbonic acid and two of hydrogen was subjected to condensation. The result was a watery vapour, and a gas of rather offensive smell." This compressed gas I found to be similar to Mr. Cruikshank's gaseous oxide of carbon from the acid air and the phlogistic air saturating each other.

Mr. Northmore apologises for giving these experiments "until he had brought them to a greater degree of perfection," but at the conclusion he also says, "Besides the above, I have made various other experiments with different gases, &c." But as he says nothing more of these *imperfect experiments*, there are no hopes of his correcting them; he appears to have exhausted his research, and we have seen with what success. There appears such an ardent desire to support the Lavoisierian theory: but if it has always failed from the experiments of Lavoisier himself, Mr. Cavendish and others, I am afraid we have little to expect from these new supporters. I might make many other observations, but these will, I presume, be thought enough.

It will be expected in contradicting Mr. N.'s experiments I should make some of my own; I must own my apparatus was not so good as his; yet I hope sufficient to prove my opinions. I had the barrel of a large blunderbuss, and stopped its priming hole, and having filled it either with sand or distilled

Experiments of
compression of
the gases.

distilled water, I then tied to its mouth a bladder filled with the different gases I wanted to compress. Upon pouring out the sand or water into the bladder, the gases entered the barrel, and then having a strong iron ram-rod made perfectly air-tight, it was forced down upon the gases by a long iron lever, by which means I was capable of making a stronger condensation than reducing them to one fifth of the volume.

They gave out heat and moisture.

Oxygen and hydrogen gave acid.

The results of these experiments were, that all the different gases, by being compressed, gave out heat and moisture: The hydrogen gas, the greatest proportion of moisture to its specific gravity. That when oxygen and hydrogen gases were compressed, there was an acid which produced flocculi in lime water; and that nitrogen gas was not necessary to the production of the acid, but rather retarded its production. The nitrogen gas obtained by the nitric acid and animal substances ought not to be used in these experiments, as it is partly acid of itself; but the nitrogen of the atmosphere ought to be made use of, being previously passed through lime water.

Mr. Nicholson, I have condensed this communication as much as possible, in order that it might not occupy too much room in your Journal. I am

Your's, &c.

H. B. K.

London, February, 15, 1806.

X.

*On the Construction of the Sails of Ships and Vessels. By MALCOLM COWAN, Esq. Captain in the Royal Navy.**

The sails of ships have been long without improvement.

It appears from the construction of the sails of ships and vessels, belonging to every nation, that it is a subject no one has hitherto taken much pains to investigate; but the maritime world have been content to use them, as they found them, every one following the beaten track of his predecessors, without examination.

* Extracted from an essay, by the author, who has letters patent for the sails.

That

That the sails of ships have been hitherto so constructed by all European nations, so as to be only managed with great labour and danger; and that when managed with the greatest skill, they are very far from being of that utility which they ought to possess, and are capable of having, is incontestible.

Ships are driven on shore every winter, which might, with proper sails, have escaped every danger. The loss of one sail, in many situations, is followed by the inevitable loss of the ship and crew. Sails are often split in hauling up to reef, and it may be necessary to reef a sail that is worn, to preserve it from splitting; hence the necessity of the sails being constructed to reef without starting tack or sheet.

They are worked with difficulty and danger.

Many ships have been lost by not having time, or drift, to haul their courses up, to reef them on the yard, by which they risk their splitting; a circumstance which alone must convince the seamen of the utility of having sails that can be reefed without taking their effect off the ship.

Many dangers may be avoided, by carrying sail with safety to the masts and yards. A ship can carry top gallant sails that reef at the foot, with safety, when other ships must furl theirs; an evident advantage in many situations.

The top sails of ships, with one or two reefs at the foot, can be reefed in a minute by one seaman at each lower yard arm, while they remain set with the top gallant sails over them, by only settling the halliards; by which a ship in squally weather, on many occasions, would have a great advantage, particularly in chase, &c. or when caught by a sudden shift of wind on a lee shore, or obliged to haul suddenly to the wind from sailing large.

Advantages of sails which are reefed at the foot instead of the head.

The facility with which sails that reef at the foot, can at all times be managed, would enable ships to make quicker voyages, and prevent them often, when weakly manned, from detaining fleets; by the difficulty and danger of carrying sail, being entirely removed, must enable merchant ships to be navigated with fewer hands, which would be a considerable saving of expence, and a great advantage in time of war in particular, when men are so scarce.

If the sails were made with horizontal cloths and seams, the sails would stand better, particularly in a gale of wind; as the strongest direction of the cloths and seams would be opposed to the greatest force of the wind, which acts horizontally;

The seams ought to be horizontal.

tally; and should the sail split in that direction, it would still remain full, and be less liable to blow away altogether, which is generally the case when a sail splits in a vertical direction. Storm stay sails set purposely with the cloths horizontal, have proved this beyond a doubt.

Many seamen are lost every winter, by falling overboard from the yards while reefing the sails, as it is more dangerous and requires longer time to perform in a gale of wind, than turling the sails, which is not so often necessary as reefing.

Other advantages from improvements in sails, &c.

Ships may sometimes avoid a lee shore, by carrying a timely press of sail, and when in that perilous situation, in a gale of wind, the safety of the ship may solely depend on the sails being kept set; though it may be necessary to reduce them, either to save them, or ease the ship. The common sails require to be hauled up, to be reefed, at the risk of splitting them, at a time perhaps, when the ship is in imminent danger, from the want of sea room; and the best seamen of the crew must be sent on the yards when they possibly may be much wanted on deck.

Whole fleets are often caught by a sudden shift of wind, of a lee shore, thrown into confusion, and obliged immediately to reef their sails, at the same time the ships may require the whole of their crews on deck, to attend the working of the ship, to keep clear of each other; particularly when it happens in the night time, with the wind squally and variable.

When ships from foreign voyages, enter the English or Irish channels, in the winter time, when the days are short, and the nights long, with weak or disabled crews, or men not accustomed to cold or frost, such as Lascars, Negroes, &c. it is with the greatest difficulty they can be prevailed on to go aloft; but should they get on a lee shore, which all ships are liable to, and with a helpless crew, nothing can exceed the horror of their situation, should they not be able to proportion their sail to the wind in time to save the ship.

Naval improvements are of great importance to the state.

To facilitate the working of ships, by the most approved means, is an object of greater consequence to a maritime nation than many are aware of, even in a commercial point of view. The little alteration that has been made in shipping for many years past, shews with what indifference attempts at improvements have been regarded; many of which have been

been tried, proved, and neglected, while others have failed from the unavoidable expence, necessarily attending all experiments on a large scale, which require repetitions to bring to perfection; or from partial interests or prejudices, being opposed with success (which not unfrequently happens) to improvements of general advantage. And many are apt to suppose that particular arts and sciences are brought to the highest degree of perfection they are capable of, though experience every day convinces us to the contrary.

The largest ships might be much more easily navigated, if the improvements on capsterns, windlasses, blocks, hawse-holes, &c. were universally adopted from the great reduction of the friction.

The following explanation will be easily understood by those who are acquainted with the construction of a ship, See Plate VI.

The courses and top gallant sails are to be reefed from the deck, and the top sails by one man at each lower yard arm.

A. The after-part of the sails.

Description of
the improved
sails.

B. A strong band on the after-part of the sails, sewed on at the upper part only, and roped at the lower part.

C. The long clews of the course, formed by the bight of the leech rope and rope of the reef band with thimbles, seized in above the tack blocks, for lashing the lower clews to.

D. The tacks and sheets fitted to the upper clews of the courses with thimbles above the tack blocks.

E. The buntlines, brought up through the thimbles H, on the foot ropes of the sails, and bent to the cringles I, on the ropes of the reef bands.

F. A small rope or gasket, rove, occasionally as a reef line, through eyelet holes, under the reef bands, and made fast to the middle sail, for confining the sail when reefed, in the wake of the reef bands.

G. Thimbles in the clews and earings.

K. Thimbles on the foot rope with the earings rove through them.

L. The reef tackle pendants, passing through thimbles in the clews and leech of the top-sail, and brought up and bent to the cringles above the upper reef band.

M. A boom tackle or burton hooked to the reef pendants.

N. The crow-foot legs to the top gallant buntline.

N. B. The reef bands are sewed by the upper part, to the after part of the sails, to prevent the rope from girting the sails, when the whole sail is set.

The rope of the reef band of the course, is the same size as the common foot rope, and the foot rope must be in proportion to the rate of the ship: for the first rates, $3\frac{1}{2}$, or 4 inch; second rates $3\frac{1}{2}$; third rates, 3 inch rope: as the quantity of sail below the reef band does not require so strong a foot rope, as when the whole sail depended on it.

The rope of the reef bands of the top sails, should be smaller than the leech ropes, as the foot of the sail will be considerably strengthened, when reefed.

These sails are not so heavy as the common ones; a 74 gun ship's course is reduced in weight about 200lbs. as the points, bands, and eyelet holes of the old reefs are not required, nor any additional gear.

Men of war will find one reef at the foot of the top sails, very useful in chace in squally weather, or when obliged to haul suddenly on a wind, &c.

Merchant ships will only require two reefs in the top sails, as the squarest part of the sail is taken off, by reefing at the foot instead of the head, but more reefs may be added if necessary.

Instructions for
reefing and
setting the
sails.

When the courses are to be reefed, cast off the lower clews, from the thimbles in the upper clews, haul up the slack sail by the buntlines, and haul tort the reef line, one part at a time, from the middle of the sail, towards the clews, and make it fast round the upper clews, so as to confine the lower clews.

To set the sail, reeve a few turns of the lashing for the clews, and haul them down, overhauling the reef line, and buntlines.

To reef the top sails, send a man up to each lower yard arm, settle the ballyards, and haul the sail down by the reef tackles, and pass the turns of the earings, through the thimbles in the earing cringles, and on the foot of the rope, and make them fast. Hoist the sail tort up, haul through the slack of the buntlines, and haul tort the reef lines on each side towards the clews, and make fast,

The top gallant sails are reefed from the deck, by the clew lines, and a single buntline with a crow-foot.

The

The buntlines and reef line will confine the slack sail, when reefed, close up the wake of the reef bands; and the buntlines will only require to be kept hand tort, as is usual, to prevent them from chafing the sail.

The slack sail of the roof of the top sail, will be kept extended tort across the foot, by the reef pendants passing through cringles in the leech.

The ends of the clewlines may likewise pass through cringles, in the leech of the top gallant sails if necessary.

The reef lines, if necessary, may be in separate pieces, made fast in the middle and quarters of the sail.

XI.

Experiments on condensed Gases. By T. NORTHMORE.

To Mr. NICHOLSON,

SIR,

I NOW take the liberty of presenting you with a con-
 tinuation of my experiments upon the condensation of the Experiments on condensed gases.
 gases, but first beg leave to make one observation, viz. that the quantity of gas said to be injected in each experiment, cannot (particularly in the preceding article) always be depended upon; for its tendency to escape is so constant and powerful, as frequently to elude every effort of mine to prevent it, and if it can find no other exit, it will sometimes escape by the side of the piston of the forcing pump. In the preceding experiments I have endeavoured as much as possible to obviate this evil, but not always with the success that I could wish.

Repeating the eighth experiment mentioned in my former letter, (see Vol. XII. p. 372-3) viz. the condensation of Nitrogen condensed upon lime, produced nitrate.
 nitrogen upon lime*, in order to discover the cause of the loss of colour in the nitrogen, I perceived that this arose from its fixation, and a nitrate of lime was the result. This experiment, on account of the elasticity of nitrogen previous to its change of habitude, requires some caution; for one of my best receivers, three-eighths of an inch thick, was

* Your marginal note says erroneously *lime-water*.

shivered in pieces with a violent explosion, after I had let it aside to see the effect of time upon the compressed gas.

Nitrogen and gaseous oxide of carbon condensed, gave nitrous acid, &c.

Experiment 9. Upwards of a pint of nitrogen was condensed, and upon this I pumped one pint of gaseous oxide of carbon. The colour of the nitrogen was destroyed; nitrous acid was formed; and upon collecting the liberated gaseous oxide, it burnt not unlike alcohol. The two gases together were at first highly elastic.

Explosions attributed to nitrogen.

From the facility with which nitrogen becomes united and fixed in various bodies, and from its expansive force when liberated from that state, I know not whether I am sufficiently warranted in suggesting an opinion, that the explosive force of various compounds may in a great measure be attributed to the sudden liberation of this fixed gas. To this cause I partly attribute the fulminating silver of Berthollet; the fulminating gold, and various nitrates; and the detonation which accompanies the decomposition of ammoniac by oxygenated muriatic acid gas.

Attempt to fire phosphorus by condensed air.

Exp. 10. Having been unsuccessful in my endeavours to inflame phosphorus by the compression of atmospheric air, (see *Exp. 4.*) I now tried oxygen, but with little better effect. The phosphorus appeared to be somewhat discoloured, and I thought had a tendency to liquify, as it does when put upon a heated plate of iron. Indeed I have no doubt that some heat is generated by the condensation of air, since the thermometer rises upon external application to the receiver.

Oxygenated muriatic acid gas gave a yellow and highly volatile fluid by condensation.

Exp. 11. Upon the compression of nearly two pints of oxygenated muriatic acid gas in a receiver two and a quarter cubic inches capacity, it speedily became converted into a yellow fluid, of such extreme volatility under the common pressure of the atmosphere, that it instantly evaporates upon opening the screw of the receiver. I need not add, that this fluid, so highly concentrated, is of a most insupportable pungency. When atmospheric air was pumped into the empty receiver, it was speedily filled with dense white fumes. There was a trifling residue of a yellowish substance left after the evaporation, which probably arose from a small portion of the oil and grease used in the machine, mixed with some of the concentrated gas; it yielded to sulphuric ether, and destroyed vegetable colours.

This

This gas is very injurious to the machine, and on that account difficult to work.

Exp. 12. Upon half a pint of oxygen was injected one pint of oxygenated muriatic acid gas. The result was a thicker substance which did not so soon evaporate, and a yellowish mass was left behind. Oxygenated muriatic acid and oxygen afforded a thicker fluid.

Exp. 13. Upon half a pint of nitrogen was injected one pint of oxy-muriatic gas. The result was a still thicker substance, and the yellow colour deeper, nor did it appear to act so powerfully upon vegetable colours. Much of the grease of the machine was carried down in both these last experiments, which formed part of the yellow residue, and yielded only to ether. Oxygenated muriatic gas and nitrogen.

Exp. 14. Having condensed about a pint of carbonic acid, the receiver very unexpectedly burst with violence. This circumstance I attribute to the vicinity of the furnace, and I mention it to guard others against standing too near a fire in these experiments; nor perhaps may it be useless to add another precaution, that of using goggles, or at least a thick plate of glass when examining the results. Receiver burst, caution.

I now took a new receiver of three cubic inches of capacity, and pumped in one pint of carbonic acid, and upon this rather more than a pint of oxygenated muriatic acid gas. Carbonic acid, and oxygenated muriatic acid.

The union produced a light sap-green colour, but no fluid, though as usual the oil of the machine had retained enough efficacy to destroy vegetable colours.

Exp. 15. Upon rather more than a pint of hydrogen, which was highly elastic, were compressed two pints of the oxygenated muriatic gas. The result was a light yellow-green colour, and no fluid. Some smoke or vapour seemed to issue out of the receiver upon turning the screw, and the gas was highly destructive of colouring matter. Oxygenated muriatic acid gas on hydrogen.

Exp. 16. I now proceeded to the muriatic acid gas, and upon the condensation of a small quantity of it, a beautiful green coloured substance adhered to the side of the receiver, which had all the qualities of muriatic acid; but upon a large quantity, four pints, being condensed, the result was a yellowish-green glutinous substance, which does not evaporate, but is instantly absorbed by a few drops of water; it is of a highly pungent quality, being the essence of muriatic acid. As this gas easily becomes fluid, there is little or no elasticity. Muriatic acid gas easily made fluid by condensation.

elasticity, so that any quantity may be condensed without danger. My method of collecting this, and other gases which are absorbable by water, is by means of an exhausted Florence flask (and in some cases an empty bladder) connected by a stop-cock with the extremity of the retort.

An idea here occurs to me, that the 'facility of' fixation which is the property of the compressed muriatic, oxy-muriatic, and some other gases, may be made of some utility to the arts, since by previously pouring in a little water, or other fluid into the receiver, an acid may be obtained of almost any degree of concentration.

Sulphureous
acid gas con-
densed by pres-
sure.

Exp. 17. Having collected about a pint. and a half of sulphureous acid gas, I proceeded to condense it in the three cubic inch receiver, but after a very few pumps the forcing piston became immoveable, being completely choked by the operation of the gas. A sufficient quantity however had been compressed to form vapour, and a thick slimy fluid of a dark yellow colour began to trickle down the sides of the receiver, which immediately evaporated with the most suffocating odour upon the removal of the pressure. This experiment corroborates the affirmation of Monge and Clouet, mentioned in Accum's chemistry, vol. I. p. 319. viz, that "by extreme artificial cold, and a strong pressure exerted at the same time, they rendered sulphureous acid gas fluid. From the injury which this gas does to the machine, it will be very difficult to perform any experiments upon its elective attractions with the other gases.

I remain, Sir,

Your obedient humble Servant,

T. NORTHMORE.

Devonshire Street, Portland Place,

Feb. 15, 1806.

XII.

On the Probability that Muriatic Acid is composed of Oxygen and Hydrogen. In a Letter from Mr. J. MARTIN.

To Mr. NICHOLSON,

SIR,

LATE experiments in galvanism have* furnished sufficient grounds to suspect, that the muriatic acid is an oxide of hydrogen, and I have been somewhat strengthened in this supposition by the well known fact, that hydrogen gas is always liberated upon effecting a solution of tin in muriatic acid: this phenomenon has been accounted for, by supposing the water which held the muriatic acid in solution to be decomposed; its oxygen seizing the metal which thereby became disposed to be taken up by the acid and the hydrogen, the other constituent part of the water being liberated under the form of gas: however plausible this hypothesis might seem, I did not think it perfectly satisfactory, for if the acid consisted of oxygen and hydrogen, part of the oxygen might unite to the metal to render it fit to be dissolved by the remaining acid, and its hydrogen of course given out under the gaseous form, in this case no decomposition of the water would take place, or at least these phenomena might happen without that decomposition. To clear up these doubts I procured an earthen tube into which was introduced some iron wire; the tube was made to traverse a furnace; to the one end was luted a bent tube, brought under the shelf of a pneumatic trough, and to the other was adapted a tubulated retort, containing some muriate of soda carefully freed from its water of crystallization. When I supposed the iron wire was sufficiently ignited, I assused some dense sulphuric acid over the muriate of soda; as soon as the atmospheric air which the vessel contained was nearly expelled, hydrogen gas was liberated from the other extremity of the tube in considerable quantities, mixed however with a small portion of muriatic acid gas; after the operation had been suffered to go on some time, the apparatus was taken to pieces, and crystals of muriate of iron were found in the tube. May

Facts induced in favour of the position, that muriatic acid may be an oxide of hydrogen.

Experiment. Muriatic acid gas disengaged from decrepitated sea salt by sulphuric acid, was passed over ignited iron. Hydrogen was liberated.

It is inferred
that this came
from the acid.

we not from this experiment be sufficiently authorized to conclude, that muriatic acid is composed of oxygen and hydrogen, and that hydrogen gas is liberated in consequence of part of the oxygen of the acid uniting to the metal to predispose it to unite to the remaining acid?

It is to be remarked, that the hydrogen gas was liberated in such abundance as to do away every idea, that it might proceed from any water which the gas accidentally held in solution.

Should you deem these observations of sufficient value, an insertion of them in your valuable journal will greatly oblige,

Sir,

Your most obedient,
and most humble Servant,

J. MARTIN.

Crown-Court, Old Broad Street,

February 20, 1806.

XIII.

*Substance of a Memoir read before the Society of Emulation, at Amiens, by Messrs. REYNARD and FACQUER, on the foul Air of Oil Cisterns *.*

Fatal effects of
the foul air of
an oil cistern.

ACHILLE POULAIN, soap-maker at Amiens, and one of his workmen having been killed by the foul air of an oil cistern, into which the latter had fallen in an attempt to cleanse it, and the former in endeavouring to save the man's life, Messrs. Reynard and Facquer were induced to make an analysis of the deleterious vapour which had caused this melancholy accident.

Dimensions of
the cistern.

The cistern measured about twelve feet in every direction. Its mouth is secured with a small cover which completely excludes the external air.

Appearance of
the oil.

The vegetable oil, of which only a small quantity at a time had been deposited in this cistern, was thick, viscid, and even in some places gelatinous, yielding a strong rancid effluvia.

* Annales de Chimie, Vol. LVI.

A lighted

A lighted candle on being let down into the cistern, was instantly extinguished.

The surface of lime-water, when included for a few minutes in a broad vessel, was slightly tinged with prismatic colours.

To obtain the gas for experiments, bottles filled with water were lowered into the cisterns, and emptied at various depths.

On the gas obtained from about two feet below the mouth of the cistern, the following experiments were tried : Observations on the gas.

1. A cylindrical vessel being filled with the gas, kept in contact with lime-water, during fifteen days, with frequent shaking, caused a small diminution in the bulk of the gas.

2. The same experiment repeated with ammoniac offered a similar result.

These two experiments denote the presence of carbonic acid gas.

3. The gas remaining from the two former experiments, when put in contact with liquid hydrogenated sulphuret of potash, underwent an absorption of eight centimes; which must have been oxygen.

The gas taken within a foot of the bottom of the cistern afforded similar results, only the proportion of carbonic acid gas was greater. That which remained after the effect of reagents was azote, as the following phenomena prove. It contained carbonic acid.

1. A lighted candle was extinguished by immersion in the gas at the upper part of the cylindric vessel; but it remained burning if the vessel was previously opened for a few seconds.

2. The vessel when reversed lost none of the gas contained in it; and the light was extinguished when introduced.

3. The luminous combustion of phosphorus in oxygen gas (the formation of nitric acid with this gas and oxygen gas not having been tried) was considered a positive proof of its nature.

This noxious gas was found to contain,

Upper Part,			Lower Part,			Analysis.
Azotic gas	-	86	Azotic gas	-	80	
Oxygenated gas	-	8	Oxygenated gas	-	6	
Carbonic acid gas	-	6	Carbonic acid gas	-	14	
100			100			
S 2			The			

Chemical
agency insuffi-
cient to destroy
the foul air.

Mechanical
means more ef-
fectual.

Destructive ef-
fects of confined
air caused by
the presence of
azotic gas.

Theory.

The nature of this gas does not admit of purification by lime or ammonia. These indeed destroy the carbonic acid, but have no influence on the azote.

Mechanical means are the only methods by which any considerable quantity of this air can be speedily removed; such as the firing of gun-powder, the use of ventilators, &c.

The result of this analysis is rather surprising, as, instead of a superabundance of carbonic acid gas, which was supposed to be the cause of the destructive effects of this confined air, azotic gas has been found—a gas lighter than atmospheric air.

The theory of this result seems to be, that the oil having deprived the enclosed air of its oxygen, leaves only the azotic gas at liberty.

XIV.

*Extrait from a Memoir, by Messrs. FOURCROY and VAUQUELIN, on the Phenomena observed in, and the Results obtained from Animal Matter, when acted upon by Nitric Acid. Read at the National Institute, by A. LAUGIER.**

Berthollet's
experiments on
azote,

—repeated.

Nitrous acid with
muscular flesh
gave azote and
some carbonic
acid.

The residuum
contained fibrous
matter, yellow
liquor, and a
greasy substance.

THE existence of azote in animal substances has been determined by the experiments of M. Berthollet, and the disengagement of this principle, when treated with nitric acid, is among the most useful of modern discoveries in chemistry.

Messrs. Fourcroy and Vauquelin, on repeating these experiments on muscular fibre, have added some interesting results to this valuable fact.

The following is a summary of their experiments, and of the results which they obtained.

SECT. 1. A mixture of 150 grammes of muscular flesh, with an equal quantity of nitric acid, at 32 degrees, and water, put into a matrafs, and heated till it boiled gently, gave 96 cubic inches of gas, containing nine-tenths of azote, and one-tenth of carbonic acid.

The residuum consisted of, 1, Matter which had not lost its original fibrous formation; 2, a yellowish liquor; 3, a greasy substance, of a yellow colour, which floated on the surface of the liquor.

After separating the greafe, and filtering the liquor, the residue was submitted to the following experiments.

To boiling water it gave a yellow colour, and the property of reddening vegetable blues: After washing in several waters, it continued to turn the colour, though it ceased to give acidity. Washing rendered its colour deeper than at first; and when diffused in a little water, it still reddened paper of turnfol.

Its solution in alcalis was of a deep blood colour. It was precipitated by acids in yellow flakes.

This matter feels fat and pitchy; has a rancid smell, and very bitter taste. The fibrous matter resembles fat.

The fusion and swelling which it undergoes when placed on hot coals, the greasy vapour, and fetid colour, produced by this operation; the small quantity of coal which it leaves, shows its resemblance to fat substances, notwithstanding its acidity.

SECT. 2. On a closer investigation of the yellow matter, the following characteristics and properties were observed:

It so saturated alcalis as nearly to mask their properties.— It saturates its combinations with potash and ammonia lathered like soap alkalies. and water, and are not decomposed by carbonic acid, but precipitated the solutions of mercury and lead in yellowish white flakes.

The yellow matter decomposed alkaline carbonates, in the cold, with effervescence, and likewise the acetate of potash, with the assistance of water, and a gentle heat. Decomposes carbonate.

The authors of the memoir next made use of alcohol, and found that the yellow matter was composed of a small quantity of fat, which was taken up by the alcohol; and of an acid, which, on account of its colour, they denominated "*yellow acid*." This acid, when deprived of its fat, which occasions an alteration in its properties, was of a deeper colour, more readily reddened the paper of turnfol, did not melt in the same manner as before, nor exhale the same rancid smell, but fetid and ammoniacal vapours. It is a yellow acid and fatty matter.

The yellow acid is dissolved in the fat, to which it communicated acidity and rancidness. It combined with ammonia, and deprived it of its smell; and by distillation it yielded all the products of animal substances. Its constituent principles, therefore, are azote, hydrogen, carbon, and oxygen; and it must be placed among animal acids. The yellow liquor is an animal acid, consisting of azote, hydrogen, carbon and oxygen.

SECT. 3.

SECT. 3. The combination of yellow acid and fat, on being again submitted to the action of nitric acid, at a temperature of about 50 degrees, underwent no remarkable alteration.— Its colour changed from yellow to white; its specific gravity was diminished, as was likewise its bulk; but without any motion or effervescence in the acid. Blue colours were deeply reddened by it; it dissolved, as before, in the ley of potash, to which it communicated an orange-red colour, and had an extremely acrid taste. The action of nitric acid upon this yellow matter seems confined to giving it properties which make it approximate to an oily state, without destroying its original acid character.

Experiments on the nitric acid wherein the muscular flesh had been decomposed.

SECT. 4. It was of importance that the nitric acid with which the muscular flesh had been decomposed, should be examined. Its yellow colour resembled that of the solution of chromate of potash. When saturated with carbonate of potash, the liquor at first acquired an orange colour, afterwards it became turbid, and deposited a small quantity of orange-red powder. On distillation, this mixture afforded a clear liquid, void of colour, of a rancid smell, containing a little ammonia, probably formed by the nitric acid. What remained in the retort, was of a blackish brown colour, but it was not farther examined.

A colourless liquor, having the same taste and smell, was afterwards obtained by distillation of another portion of the nitric acid used in the decomposition of the muscular flesh. The liquor remaining in the retort became yellow by concentration, and its re-action upon nitric acid was quickly perceived in a copious emission of red vapours. When reduced to 40 grammes, flattish crystals were formed in a thick mother-water, whose tenacity was similar to that of the solution of gum.

This mother-water possessed an acid bitter taste, and on the addition of a little caustic potash, became of a blood-red colour: mixed with alcohol, it deposited a white flaky sediment, which afterwards formed itself into fine semi-transparent grains, of a pleasant acid flavour.

Five decigrammes of this salt, on being calcined, left 21 centigrammes of yellowish very light residuum, which effervesced and were dissolved in nitric acid, and on being evaporated produced crystals of sulphate of lime and nitrate of potash.

This saline precipitate, obtained by means of alcohol, was ascertained to be a mixture of sulphate of lime and acidulous oxalate of potash.

The mother-water, after precipitation with alcohol, gave a second precipitate with lime-water, consisting of oxalate of lime. After this double operation with alcohol and lime-water, the mother-water, on being gradually evaporated, became converted into the brown viscid syrup, of a bitter taste, like that of walnut shells. This being mixed with a good quantity of alcohol, coagulated, and threw down a plentiful precipitate of white matter. This matter was very pure malate of lime, the alcohol having retained the yellow acrid substance.

The learned authors of the memoir, of which we have given this detailed extract, conclude from the facts above stated, Conclusions.

1. That the muscles contain potash, lime, and sulphuric acid, or perhaps sulphur burned by nitric acid.

2. That a portion of the muscular fibre, or rather the cellular membrane with which it is enveloped, was converted by the action of the nitric acid into oxalic acid and malic acid.

The alcohol employed in the separation of the malate of lime, held in solution, 1. A small portion of nitrate of lime; 2. A very bitter red-brown matter, possessing the flavour of walnut rhinds, of which more will be said hereafter; 3. A small quantity of that detonating matter already found in indigo: it was in this case obtained by concentrating the alcoholic solution, and separating it by the addition of carbonate of potash, in the form of granulated crystals, very inflammable, and very detonating.

SECT. 5. The importance of the results obtained from the foregoing analysis will be readily understood; particularly if a comparison be made of the knowledge hitherto possessed, with the extensive notions here opened to the view, of an object so interesting in the consequences which may be drawn from it, in the applications which may be made to the animal economy, and which, as will be shewn, leaves scarcely any thing more to be desired. Importance of the foregoing analysis.

The disengagement of azotic gas, the formation of carbonic acid, of fat, of oxalic acid, and of a bitter substance, constitute the whole that was known respecting the treatment of animal subject. Discoveries added to what was formerly known on this animal subject.

animal substances by nitric acid; to this is now added the discovery, 1, Of a yellow insipid matter, of little solubility, though acid, and which immediately succeeds the fleshy fibre; 2, Of another yellow matter, bitter, more soluble, and equally acid, which remains dissolved in the nitric liquor; 3, Of an inflammable, detonating substance, which is also retained in solution; 4, and lastly, of the formation of malic acid.

It appears, and is the opinion of Messrs. Fourcroy and Vauquelin, that the yellow and nearly insoluble matter is the first degree of change produced upon the muscular fibre; it passes quickly to the second degree of alteration and of acidity, whose product is the more soluble yellow matter: this, by a third degree of alteration is succeeded by the inflammable detonating substance, being the third and last term of the decomposing action of nitric acid. The authors of this memoir attribute the successive formation of these three compounds to the subtraction of part of the azote, and of a more considerable portion of the hydrogen: by this means the proportions of their elements are changed, and there remains an excess of carbon and of oxygen, which produces the state of fat and acidity already noticed. As to the proportion of the constituent principles of these three compounds, it is a problem of too remote a nature for its solution to be readily discovered.

Acidity of the yellow substance not caused by nitric acid.

Messrs. F. and V. examined if the acidity of the yellow substances might in any measure arise from nitric acid; but, after a careful investigation, they were satisfied that it was in no degree present,

Formation of oxalic and malic acids.

The formation of oxalic and malic acids belongs to the white mucous scales of the cellular membrane. Comparative experiments of the effects of nitric acid on the white membranaceous organs, which furnished plenty of these acids, and very little of the fat yellow matter, led the authors to this conclusion.

SECT. 6. A few insulated facts, which hitherto have scarcely appeared to be susceptible of any useful application, seem to unite with those presented by this analysis; and the learned chemists, to whom we are indebted for it, have not omitted to connect them with the other facts. Such are those which are obtained by examining the bilious concretions in certain animals; those in the gall-bladder of the ox and elephant; and the analogy which appears to exist between bile,

the

the colour of the skin in persons afflicted with the jaundice, and also their urine, and the yellow substance treated of in this memoir.

Analogy of the yellow matter to bile, jaundice, &c.

New experiments made with a view to confirm these suspicions obtained the most happy results. The red matter of bilious concretions, when separated from the bitter green matter with which it is combined, displayed similar properties with the first yellow matter obtained from muscles acted upon by nitric acid.

Bilious concretions.

From the urine of a young man troubled with a slight jaundice, they obtained a red substance, whose identity with the matter formed by muscles and nitric acid was remarkable. To obtain this, they evaporated the urine to the consistency of honey, and treated the residuum with alcohol: this contained, besides much of uree, sal-ammoniac, and acetate of soda, of which the patient made use, the red substance they sought for.

It was found in the urine of an icteric subject.

From these experiments, made with skill and ability, may we not conclude with the authors, that the jaundice is occasioned by a superabundance of this matter introduced to the cutaneous absorbent system; that this is what gives a yellow colour to bile and bilious calculi, which display, on analysis, the same properties; and that the yellow acid is dispersed throughout the animal economy, either by the oxygenation of the muscular fibre, or of the sanguineous fibrine, from which it is formed?

Jaundice occasioned by a superabundance of the yellow acid; which also causes the yellow colour of bile, &c.

Neither can we avoid admitting a striking analogy between this yellow acid matter, and the acid found in fat after long exposure to the air, or that has contracted a yellow hue through disease, and fat treated with nitric acid to form oxygenated pomatum.

Resemblance of the yellow acid and rancid matter of fat.

It must be confessed that these conjectures assume much probability, when we consider that the acetate of soda, alkaline carbonates, and yolks of eggs, are the remedies best adapted for the cure of the jaundice, and form also the best chemical solvents of the yellow acid, or of the acid and fat matter, which so evidently characterise the jaundice.

Other facts.

After what has been said, it must no longer be imagined that the hope of tracing the cause of morbid affections, is altogether chimerical: nor that discoveries in chemistry, and attentive

Chemical researches not to be neglected by physicians,

researches

researches respecting animal matter, will not enlighten the physician on the nature of diseases, and the means of curing them.

XV.

Remarks relative to Dr. HERSCHEL'S Figure of Saturn.

By AN OBSERVER.

To Mr. NICHOLSON.

SIR,

ON reading in your Journal, Observations on the singular Figure of the Planet Saturn, by Dr. Herschel, from the Philosophical Transactions; when I saw the engraving of the figure, as described by the Doctor, resembling a parallelogram, one side whereof is the equatorial and the other the polar diameter, with the four corners rounded off, so as to leave both the equatorial and polar regions flatter than they would be in a regular spheroidal figure; I was surprised to find, on enquiry, that so remarkable a figure had not been noticed before by other astronomers, whose telescopes were supposed to define objects very correctly, with powers considerably exceeding 160 times, by which power the Doctor could distinguish Saturn from the spheroidal figure of Jupiter.

Former obs. of the Doctor did not shew it.

In the year 1776, the Doctor relates he perceived the body of Saturn was not exactly round, and in 1781, that it was flattened at the poles, at least as much as Jupiter. In 1789, the Doctor being then prepossessed with its being spheroidal, he measured the equatorial and polar diameters, and supposed there could be no other particularity to remark in the figure of the planet.

It is evident, from the Doctor's former observations of Saturn and Jupiter, that the visible difference in their figures was not, before last year, observed so distinctly, owing to the superior excellence of his 10-feet telescope of two feet aperture, but that, when observed, he afterwards found the other telescopes gave a similar disparity.

Q. whether there was no deception in the telescopes.

As the figures given by former astronomers, and even by the Doctor himself, of both Jupiter and Saturn, were spheroidal.

dal, it may be requisite, before any intricate researches are attempted (as mentioned by the Doctor at the end of the communication), to be well assured that his telescopes have defined the figures of the planets accurately, which at present admits of a doubt, and which may be cleared up about the time of the next opposition of the Sun and Saturn, in April next.

The following may prove the necessity of such an enquiry :

Place a circular or spherical figure before a concave mirror, Experiment.
 which mirror must be so inclined, that when the object is An object seen by oblique reflection from a spherical mirror, is rendered oblong.
 above the head of the observer, it may be seen, by reflection, in the center of the mirror * : If seen within the focus, the object will be represented oval in a vertical direction, and when beyond the focus, in a horizontal ; which figure will be more and more oval as the angle is enlarged.

Your's,

AN OBSERVER.

XVI.

Experiments on a Mineral Substance formerly supposed to be Zeolite ; with some Remarks on two Species of Uran-glimmer.
 By the Rev. WILLIAM GREGOR.†

THIS mineral is raised in a mine called Stenna Gwyn, in the parish of St. Stephen's, in Branwell, in the county of Cornwall ; the principal production of which is the compound sulphuret of tin, copper, and iron. Description and analysis of a mineral from Cornwall.

Description.

Two species of this mineral are found, assuming a marked difference in external character.

The first and most common one consists of an assemblage of minute crystals, which are attached to quartz crystals, in tufts, which diverge from the point of adherence, as from a centre. These tufts vary, as to the number of crystals, of which they

* If the object is small, it may be enlarged by a concave eye-glass.

† Phil. Trans. 1805.

Description and
analysis of a
mineral from
Cornwall.

are composed, and are light and delicate in the forms which they assume, or they are grouped together according to a variety of degrees of proximity and compactness. Sometimes they fill the whole cavity of a stone, with little or no interruption; in other specimens they are seen partially spreading over the sides and pointed pyramids of quartz crystals.

In some cases these grouped tufts adhere very pertinaciously to the stone which bears them; in others, they are easily separable, in comparatively large pieces, from the quartz, the impressed form of which the pieces thus separated retain. The surface of these, which was in immediate contact with the quartz, exhibits the several minute crystals of which the mass consists, matted together in various directions.

These crystalline assemblages are, in general, white; a nearer inspection of the individual crystals proves that they are transparent. Sometimes they are stained of a yellowish hue by ochry water.

The size of these crystals varies considerably in different specimens. Sometimes they assume the appearance of a white powder raised up in small heaps, upon the surface of the stone, to which they adhere. In other specimens they resemble a tender down. And the larger sort varies, in relative size, in the proportion, perhaps, in which a human hair, horse-hair, and a hog's bristle, severally differ from each other in magnitude. They seldom exceed a quarter of an inch in length. The figure of these crystals is not easily ascertainable, on account of their minuteness. By the help of a very powerful microscope, they appear to consist of four-sided prisms; where these are broken off, the section exhibits a rhomboidal, approaching indeed to an elliptical figure, from the circumstance of the angles of the prism being worn away; but that the prism itself is rhomboidal, cannot be inferred from hence, unless we could be certified, that the section were at right angles with the axis of it.

Imbedded amongst these crystals two species of crystalline laminæ are frequently discoverable; the one consisting of parallelepipedon plates with truncated angles, applied to each other, of a green colour of various tints, from the emerald to the apple-green: the other species, consisting of an assemblage of square plates, which vary in thickness. The angles of the several square laminæ, which are applied to each other, are

no.

not always coincident. They are of a bright wax yellow. The sides of the largest of these square laminæ is about a quarter of an inch. This last species is frequently found adhering to the sides of quartz crystals, in the cavities of granite.

Description and
analysis of a
mineral from
Cornwall.

The other species of this mineral consists of an assemblage of crystals closely compacted together in the form of mamillary protuberances, in general, of the size of small peas, intimately connected with each other. A stratum of these about $\frac{1}{4}$ of an inch thick, is spread upon a layer of quartz, in the cavities or fissures of a species of compact granite. The strizæ of which these mamillæ consist, diverge from a centre, like zeolite. Some of the individual strizæ, in some cases, overtop their fellows, in these globular assemblages, and evidently assume, on their projecting points, a crystallized form.

A.

(1.) The detached crystals of the former species are easily reduced to powder, of a brilliant whiteness. At the temperature 56° of Fahrenheit, its specific gravity was found to be 2,22.

(2.) The hardness of the more compact species is sufficient to scratch calcareous spar. At the temperature 55° , its specific gravity was 2,253. It does not imbibe water.

(3.) Some of the crystals exposed, on charcoal, to the flame of the blowpipe suddenly and strongly driven upon them, decrepitate: if they are gradually exposed to the flame they grow opaque, and become more light and tender: but they show no signs of fusion under the strongest heat.

(4.) The phosphate of soda and ammonia takes up a piece of this mineral without effervescence, but it swims about the fused globule, unaltered. Borax dissolves a fragment of a crystal, and the globule remains transparent.

(5.) Some of this mineral, reduced to a fine powder, was mixed with about half its weight of pounded quartz, and kneaded with water into a ball: but as soon as the mass became dry, all cohesion was destroyed, and it fell into powder.

(6.) Sulphuric acid, poured upon some of it, caused no effervescence, nor was there any perceptible vapour extricated.

(7.) Some of the pulverized crystals were put into a crucible of platina, and sulphuric acid was poured upon them. The

Description and
analysis of a
mineral from
Cornwall.

crucible was covered with a piece of glass, and placed in warm sand. On examination of the crucible and its contents, after some time, it appeared that the greater part of the mineral had been dissolved, but the surface of the glass cover was not in the least affected.

(8.) Some of the crystals were introduced into a small glass retort, to which a receiver was adapted. The retort was exposed to the heat of a charcoal fire. A fluid distilled over into the receiver, which had a peculiar empyreumatic smell. It changed litmus-paper to a faint red. It produced no change in a solution of nitrate of silver; but it caused a white precipitate in a solution of nitrate of mercury. I attributed these phenomena, at the time, to a small bit of the feather with which I had swept the powder into the retort, and which, I thought, had fallen into it. A slight whitish crust was also produced in the neck of the retort, but the smallness of the quantity did not admit of examination.

(9.) Some of this mineral, exposed to a red heat for about ten minutes, lost in weight at the rate of $25\frac{1}{2}$ per cent. Another portion, exposed to a stronger heat for more than an hour, lost $30\frac{1}{2}$ per cent. This operation was performed in a crucible of platina; the cover of which gave some indications as if a slight portion of the finer parts had been volatilized.

Some of the compact species, after exposure to a red heat for one hour, experienced a diminution in weight of 30 per cent.

(10.) The sulphuric, muriatic, and nitric acids, aided by a long digesting heat, effect nearly a complete solution of this substance. The quantity of the undissolved residuum is diminished in proportion to the purity of the mineral employed.

(11.) The nitrate of silver, as well as the muriate of barytes, produce no change in the solution of this substance in nitric acid.

(12.) The solutions of this substance in muriatic and nitric acids, cannot be brought to crystallize.

B.

(1.) I selected some of the crystals of this substance, as free as it was possible from extraneous matter. 50 grains grossly pounded were exposed, in a platina crucible, to a red heat for

one hour. They weighed, *while still warm*, $35\frac{1}{2}$ grains, which is a loss of $28\frac{1}{2}$ per cent. 25 grains of the same parcel, from which I had taken the former, exposed to a heat of longer continuance and greater intensity, were diminished in weight, at the rate of $30\frac{1}{2}$ per cent.

Description and
analysis of a
mineral from
Cornwall.

(2.) The powder still preserved its pure whiteness. It was transferred into a matrafs, and nitric acid poured upon it, which soon began to act upon it. The matrafs was placed, for many hours, in a digesting heat. A solution of the whole of the substance, except a small portion, was effected. I added a few drops of muriatic acid, and continued the digestion.

(3.) The acid was now diluted with distilled water, and poured off from the residuum, which consisted partly of a fine spongy earth, and partly of fragments of quartz. It was caught on a filter and sufficientlyedulcorated. The last portion ofedulcorating water dropped through the filter of an opalish hue.

The residuum, dried and exposed to a red heat, for ten minutes, $= \frac{1}{12}$ of a grain, $\frac{1}{12}$ of which consisted of fragments of quartz, $\frac{1}{32}$ was found to be filica, and $\frac{1}{32}$ alumina.

C.

(1.) The clear solution andedulcorating water were poured into a large matrafs and boiled, and whilst boiling, the contents were precipitated, in white flakes, by ammonia

(2.) When the ammonia had ceased to produce any further precipitate, the clear fluid was decanted, and assayed with carbonate of ammonia. But its transparency was not in the least disturbed.

(3.) This clear fluid, together with theedulcorating water, with which the subsided precipitate had been washed, was gradually evaporated. When its volume was considerably diminished, a separation of a spongy earth took place, more copiously than I had reason to expect, and the quantity of it was still further increased by a few drops of ammonia. This earth, thus separated, was sufficientlyedulcorated, and added to the former precipitate.

(4.) The fluid was again evaporated, and at last transferred to a crucible of platina, and the salt reduced to a dry state: on redissolving this salt in distilled water, a minute portion of

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earthy matter was separated, which, after edulcoration, was added to the rest. The fluid from which it had been separated, and the edulcorating water, were again evaporated to dryness, and the ammoniacal salt expelled by heat, in a platina crucible.

(5.) After the crucible had been made red hot, it was examined. I discovered on the bottom of it, some traces of earthy matter, and some spots, which had a glassy appearance. Water boiled upon it, dissolved nothing; from which circumstance, the absence of both of the fixed alkaline salts may be inferred. Neither did nitric acid produce any alteration. A few drops of sulphuric acid effected a solution of the substance, which adhered to the bottom of the crucible. Ammonia precipitated from it a small quantity of earth, which was transferred to the rest, and the sulphate of ammonia and edulcorating water were again evaporated and expelled by heat. A few spots of the same glazing still appeared. I had observed the same phenomenon in a former experiment: but in that, as well as in the present instance, the substance was in too small a quantity to become the subject of experiment.

D.

(1.) Upon the precipitate (C 1), and the earths collected at different times, whilst they were in a moist state, I poured a solution of potash in alcohol mixed with distilled water; in a short time, the greater part of it was dissolved.

The clear solution was decanted, and the undissolved sediment was transferred to a basin of pure silver, and boiled with a solution of potash.

(2.) When the potash ceased to act upon it, it was diluted with distilled water and decanted from a brown powder, which had subsided. This powder edulcorated, dried, and ignited weighed $\frac{7}{16}$ of a grain; $\frac{1}{4}$ of a grain was alumina, $\frac{1}{12}$ silica, and $\frac{1}{2}$ oxide of iron.

E.

(1.) The solution effected by potash was decomposed and redissolved by muriatic acid, and the contents of the solution were precipitated by ammonia. The subsided precipitate was edulcorated.

(2.) The fluid and the edulcorating water were evaporated to dryness, and redissolved in distilled water. Here again, to my surprise, a separation took place of a white earth, more abundant than is usual in cases where ammonia is employed as a precipitant.

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(3.) This earth and the precipitate were edulcorated with distilled water, until it ceased to affect a solution of nitrite of mercury. Collected, dried, and ignited, for one hour it weighed *whilst still warm* $32 \frac{1}{8}$.

F.

(1.) This earth was placed in a crucible of platina, and repeatedly moistened with sulphuric acid, which was abstracted from it in the sand bath; distilled water affected the solution of the whole, except a white powder which weighed, after ignition, $2 \frac{1}{2}$ grains. It was proved to be silica.

(2.) This solution was now mixed with some acetat of potash and gradually evaporated; large and regular crystals of alum were from time to time formed. A small portion of silica which weighed after ignition $\frac{1}{8}$ of a grain was deposited; some sulphate of lime also made its appearance, which washed with diluted alcohol and dried in a low heat $\frac{7}{8}$ of a grain.

(3.) A portion of the fluid remained which neither the addition of potash nor the lapse of many weeks could induce to crystallize. Suspecting that it might contain glucine, I precipitated the contents by carbonate of ammonia, added to excess, and shook the mixture repeatedly and strongly. The precipitated earth was collected and the fluid boiled, but it was found to contain nothing but a minute portion of alumina.

(4.) The edulcorated earth was redissolved in sulphuric acid, except $\frac{1}{8}$ of a grain of ignited silica.

The solution was mixed with a little potash, and gradually evaporated. Sulphate of lime was separated at several times and after long intervals, which sufficiently washed and dried in a low heat = $\frac{7}{8}$. Some silica also separated, but too minute in quantity to be ascertained by weight. The remaining fluid at length crystallized into regularly formed alum.

(5.) The whole, therefore of the $32 \frac{1}{8}$ (E. 3.) consisted of alumina except $2 \frac{1}{8}$ of silica, and the lime contained in $\frac{7}{8}$ of sulphate of lime, which may be estimated about $\frac{1}{8}$ of a grain; the alumina, therefore, = 29; the alumina in B. and D. = $\frac{1}{2}$.

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the silica in B, D, and F, = $3\frac{1}{8}$; the oxide of iron (D.) = $\frac{1}{4}$, and lime F, $\frac{1}{8}$; the volatile parts of this substance = $15\frac{3}{8}$ in the 50 grains employed.

The sum total of these is	-	-	-	47 $\frac{1}{8}$
Loss	-	-	-	2 $\frac{1}{8}$

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I have subjected these crystals, as well as the harder species of this mineral, to analysis by means of direct solution in sulphuric acid, and have found in each case the same fixed ingredients, viz. alumina, a small portion of silica, and a very minute quantity of lime. Both these latter ingredients are, I think, essential to the composition of this fossil, as I have always discovered them in the purest specimens. In this mode of analysis I experienced the same difficulty and tediousness of delay in bringing the last portions of the solution to crystallize into alum. This anomalous circumstance I have reason to attribute to a particular combination, which takes place between the sulphate of alumina and lime, silica, and potash. In my examination of the compact species there was no appearance of the sulphate of lime until the last; and in every experiment, previously to the fresh appearance of crystals of alum that had been long delayed, silica and sulphate of lime were deposited.

I forbear entering into any further details concerning my former experiments on this curious fossil, as I have reason to think that it will still require a more particular and minute examination, on account of another ingredient which eluded my notice, and which may possibly impart to its peculiar character. The scarcity of it has been hitherto a great bar to my experiments; I shall record, however, a few facts which I have lately observed, in the hope that at a future time I may be able to resume my examination of it.

I was induced to pay more attention to the volatile ingredients of this substance*. With this view, I introduced some

* Mr. Humphry Davy, whose well known skill and sagacity have probably rendered the researches of another person superfluous, had, I found, been engaged in the analysis of a mineral which is thought to be identical with the subject of these observations. He informed me that he had observed a peculiar smell, and acid properties in the water distilled from the substance which he examined.

of the crystals into a small retort, adapted a receiver unto it, and exposed the retort to a charcoal fire. The neck of the retort was soon covered with moisture, which passed into the receiver; and I observed a white crust gradually forming in the arch and neck of the retort.

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Cornwall.*

On examination of the fluid in the receiver, it was found to have the same empyreumatic smell that I had observed before. It resembles very much the smell which that fluid is found to have which is distilled from the white crust that surrounds flint as a nucleus.

It changed litmus paper to a faint reddish hue. It produced no change on a solution of nitrate of silver, and scarcely a perceptible one, on that of nitrate of mercury.

The crust formed in the neck of the retort consisted of thin scales, which after the vessel had been dried, were disposed to separate from the glass in some places, but in others they firmly adhered unto it. They were opaque, like white enamel, and reflected the colours of the rainbow. A portion of this substance exposed to the flame of the blow-pipe upon charcoal turned at first black, and then melted into a globule, that exhibited somewhat of a metallic splendor which soon grew dull. This substance is soluble in water; on evaporation of it, it assumes, at the edges of the fluid, a saline appearance, which, as the moisture evaporates, becomes earthy, opaque, and white. Some of the solution changed litmus paper to a faint red. Lime and strontian waters produce in it white clouds, which a drop of nitric acid removes. Murriats of lime and barytes produce no change in it. Nitrate and acetate of barytes disturb its transparency, the effect produced by the latter is more evident. Nitrate of silver produces no effect, but nitrates of mercury and lead cause copious precipitates, which are white and soluble in nitric acid. Phosphate of ammonia and soda produced a white precipitate. Oxalate, tartrate, and prussiate of potash did not affect it, nor did sulphate of soda. Ammonia was dropped into it, but the fluid preserved its transparency. But carbonate of ammonia instantly caused a white precipitate, which was not redissolved by an excess of the precipitant; upon some of this subsided precipitate a concentrated solution of potash was poured and shaken with it, but it was not

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sensibly diminished. But if afteredulcoration it be dissolved in nitric acid, and potash be added, no precipitate is produced.

Carbonate of potash causes a white precipitate when dropped into the aqueous solution of the scaly sublimate.

The supernatant fluid was poured off and gradually evaporated, but it became repeatedly turbid, nor could I by means either of the filter or alcohol prevent a recurrence of the same effect. Nearly the same result takes place when carbonate of ammonia is used as the precipitant.

Some of the white scales were moistened with sulphuric acid. No vapour arose.

Some of the precipitate obtained by means of carbonate of potash from the watery solution of this substance, was, after sufficientedulcoration, dissolved in sulphuric acid; the solution, on due evaporation, produced permanent crystals, some of which resembled alum, but others seemed to differ from it in external character. Ammonia decomposed the solution of them in water, and a few drops of liquid potash dissolved the precipitated earth. The quantity was too small for further experiment.

If distilled water be poured into the retort and boiled in it, so as to dissolve what adheres to the neck and cavity of it, a further solution is effected, but differing in some measure from the solution of the sublimate collected from the neck of the vessel. This latter solution is found to contain lead. If nitric, or muriatic acid be poured into the retort, so as to dissolve what still remains adhering to it, the presence of lead becomes more evident. Whence does this metal arise? I have reason to believe that it arises from the glass retort, which is corroded by the acid of the fossil extricated by heat. But what acid is it? It does not seem to be either the phosphoric or fluoric acids, the latter of which became the first object of my suspicion.

The opinion which Mr. Davy suggested to me seems more probable, that it is of vegetable origin. Oxalic acid, on the authority of Bergman, may be volatilized; yet some of its properties are very extraordinary and do not accord with this idea.

I decomposed the watery solution of the scales by nitrate of lead, and after a sufficientedulcoration of the subsided precipi-

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tate, I dropped upon it some sulphuric acid. No fumes were perceptible. The sulphate of lead was separated by the filter, and the clear fluid, which passed through it, was gradually evaporated; small crystallizations were formed, the figure of which I could not ascertain; some of them were exposed to the flame of the blowpipe in a gold spoon; they did not burn to coal, nor give out any empyreumatic smell nor fusc, but they assumed an earthy appearance*.

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Uran-glimmer.

I shall add a few desultory remarks upon the yellow and green crystals, which frequently accompany the fossil.

I considered them to be the two species of *uran-glimmer* which had been examined by the celebrated Klaproth.

The yellow cubic crystals are light. Their specific gravity, taken at temperature 45° Fahrenheit, was 2,19.

Exposed to the flame of the blowpipe on charcoal, they decrepitate violently. A piece of this substance is taken up by phosphate of ammonia and soda, without effervescence, and communicates a light emerald-green colour to the fused globule.

By exposure to a red heat, this substance loses nearly a third part of its weight. It then becomes of a brassy colour.

It is soluble in the nitric and muriatic acids; but I could procure no crystallized salt from the solution of either of them.

By evaporation to dryness, and redissolving the mass, some silica is separated.

A

(1.) A certain quantity of the yellow crystals were dissolved in nitric acid. Muriatic and sulphuric acids successively dropped into the solution produced no sensible change. The contents of the solution were precipitated by ammonia,

* I subjected some of the Bainsdale mineral, with which Mr. Raffleigh kindly furnished me out of his cabinet, to experiment, with a view of ascertaining whether it would produce the same volatilized saline crust, as the *stenna gwyn schist*, and I found that it did.

in

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in white clots, mixed with some of a yellowish hue. Ammonia, added in excess, betrayed no sign of the presence of copper.

(2.) The ammonia, on evaporation, was found to have held a portion of the mineral in solution. A fresh portion of ammonia dissolved more, but in a less quantity, at each succeeding affusion of it.

(3.) The precipitate, which had resisted the ammonia, was boiled in a silver crucible, with a solution of potash in alcohol, diluted with distilled water, and a considerable portion of the substance was dissolved by it: the potash and the ammonia had dissolved rather more than half of the fixed ingredients of it.

(4.) The edulcorated residuum, which was of a dirty yellow colour, was transferred to a crucible of platina, and moistened with sulphuric acid, which was abstracted from it, in the sand-bath. The brownish-gray mass was elixated with distilled water, which dissolved nearly the whole of it. The residuum consisted of a white heavy powder, which, tried in different ways, was found to be *sulphate of lead*.

(5.) The solution effected by sulphuric acid was greenish. On evaporation, a salt was produced, of uncommon brilliancy, resembling scales of mica, or silver leaf. These diminished in quantity at every fresh solution and evaporation, and at last they could not be reproduced; but a confused crystallized mass remained. How far the platina crucible may have contributed to this phenomenon I cannot ascertain.

(6.) The solution of the saline mass was precipitated by potash, of a dark brown colour. The potash held nothing in solution. I redissolved the precipitate in nitric acid, and precipitated the solution by ammonia, of a bright yellow colour, peculiar to the *oxide of uranium*, with which it agreed in other properties.

(7.) What was dissolved by ammonia (2.) amounted to nearly $\frac{1}{2}$ part of the fixed ingredients. It was white, inclining to ash-colour. It tinged phosphate of soda and ammonia of a light green. It was soluble in sulphuric acid, except a few gelatinous flakes. The solution was greenish; gradually evaporated, it shot into a number of minute stilted crystallizations, which were circular, and consisted of rays diverging from a centre. They were, in general, colourless; a few of

them were tinged of a smoke-colour. They soon became deliquescent. Upon evaporation, the same crystallizations were produced. After a time, some detached, regular, and permanent crystals were formed, which were colourless. Their figure I could not accurately ascertain. They were exposed to a red heat in a platina crucible. No ammoniacal vapour was perceptible. The crystals melted into opaque globules; some of these were transferred to a small glass, and distilled water was poured upon them. No solution took place apparently: on shaking the glass, the globules fell to pieces into gelatinous flakes, which were white. Some of the supernatant fluid was tried with muriate of barytes, which produced a cloud. But neither ammonia nor prussiate of potash caused any change in it. It is soluble also in nitric acid: the solution formed a confused crystallized mass, which soon became deliquescent. Zinc, immersed in it, caused the separation of white gelatinous flakes. Iron caused no change. Ammonia and potash threw down white precipitates, a portion of which were redissolved. The carbonates of soda, potash, and ammonia produced white precipitates. Prussiate of potash threw down the contents of the solution in distinct flakes, of the colour of mahogany; and the solution of galls in alcohol caused a light yellow powder to subside. It is soluble also in muriatic acid; the solution is a very dilute green. It requires an excess of acid to hold the substance in solution: which, after a time, deposits crystalline grains of a yellowish colour, which require a large quantity of water to dissolve them.

Acetic acid does not dissolve this powder.

(8.) What was dissolved by potash (3.) was of an isabella colour; it was tried with nitric, muriatic, and sulphuric acids, neither of which could dissolve the whole of it. What resisted the two former acids was found to be silica. That which remained undissolved by the latter, was silica and sulphate of lead. Evaporation of the latter solution, betrayed also the presence of lime, in the state of sulphate. The nitric and muriatic solutions, on evaporation, deposited nitrate and muriate of lead; and sulphuric acid dropped into them produced a small quantity of sulphate of lime.

The nitrate and muriate of lead were decomposed by sulphuric acid, and the lead reduced on charcoal.

Ammonia

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Ammonia precipitated what remained in these solutions, and redissolved a part of the precipitates, which agreed in properties with that substance before mentioned (2.); the remainder was of a brighter yellow. But I could not bring the solution of it in nitric acid to crystallize.

B.

(1.) Some of the yellow crystals, which had not the slightest appearance of being contaminated with extraneous matter, were dissolved in sulphuric acid. *Silica* was separated; and the presence of *lime* and *lead* proved by the appearance of their respective *sulphates*.

(2) If sulphate of ammonia is dropped into a solution of this mineral in nitric or muriatic acids, no change takes place, *immediately*. But on evaporation, a yellowish crust is deposited, which is insoluble in water. A solution of carbonate of soda in water, boiled on it, becomes yellowish-brown, and the greater part of it is dissolved. The residuum, which is white, is reduced on charcoal to a globule of lead. What the carbonate of soda had dissolved was found to be *oxide of uranium*. Sulphuric acid *alone*, does not produce this deposited crust.

(3) Some perfectly pure crystals were dissolved in muriatic acid. Some silica was separated. A few drops of sulphuric acid were dropped into the solution, which produced no immediate change. on evaporation a white powder, separated, which consisted in part of sulphate of lime. The remainder, exposed to the flame of the blowpipe, was reduced to globules of lead.

The solution was decomposed by ammonia, which redissolved a part of the precipitate; and, afteredulcoration, the precipitate was dissolved by nitric acid, and precipitated again by ammonia, which held a less quantity in solution. Theedulcorated precipitate was now boiled with a solution of carbonate of soda, which dissolved a large portion of it. The solution was yellowish-brown, and contained oxide of uranium. What was undissolved by the carbonate of soda was dissolved in sulphuric acid, and seemed to be the same substance as that which the ammonia held in solution.

A. (2)

The

The scarcity of this beautiful mineral has precluded me from operating on such a sufficient quantity, as a regular and rigid analysis required.

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The substance, which is held in solution by ammonia, has some peculiar properties that seem to distinguish it from uranium. And if this mineral be the uran-glimmer, I have certainly detected the oxide of lead, lime, and silica in it, which have not hitherto been considered as ingredients of that fossil. The green crystals differ in no respect from the yellow, except in containing a little of the oxide of copper.

XVII.

*Examination of different Methods of separating Nickel from Cobalt. By M. C. F. BUCHOLZ.**

THE want of nickel and cobalt in a state of purity induced M. Bucholz, to make experiments himself on the means of procuring them, and to repeat those of others.

A. The able chemist Hermstadt proposed to separate oxide of cobalt and oxide of nickel, by dissolving the nitrate or sulphate of cobalt, impregnated with nickel, in ammonia, and exposing the solution to a single evaporation. This M. Bucholz tried in the following manner, for the reversed purpose.

M. Bucholz repeats Hermstadt's method.

1. An ounce of cobalt ore (*cobalt spec*) was dissolved with heat in four ounces of nitric acid of the specific gravity 1,220, and mixed with an equal quantity of water; which produced a residue of three drams of oxide of arsenic, in the form of small crystals. When the solution mixed with half the quantity of water, coloured of a dull green, had been filtered and diluted with a great quantity of water, it deposited a little of the oxide of bismuth. Caustic ammonia was then mixed with it to excess, until no farther apparent solution took place of the precipitate obtained. That which was not dissolved, of a dull reddish white, was a composition of arseniate of cobalt with a little of the oxide of bismuth, and the oxide of iron.

One oz cobalt ore dissolved in nitric acid. Deposits 3 drams of arsenic.

The solution filtered and diluted deposits a little bismuth oxide. Caustic ammonia added. The undissolved residue is arseniate of cobalt, with oxides of bismuth and

* Bucholz, &c Journal of Chem. III. p. 2.

By evaporation
oxides of cobalt
and nickel are
precipitated.

The solution being filtered, appeared of a beautiful blue, it was then evaporated at a gentle heat, by which about two drams of a bright green precipitate were obtained; which proved to be oxide of nickel, united to oxide of cobalt. The filtered liquor being then afterwards evaporated at the heat of a stove, deposited still an oxide of the same quality.

The saline mass
obtained, re-dis-
solved, filtered,
and boiled with
caustic potash
produces pure
oxide of nickel.

The saline mass of ammoniacal nitrate of nickel, of a deep green colour, which had been obtained by the evaporation, was re-dissolved, filtered, and kept in ebullition with an excess of caustic potash, until the evaporation of the ammonia was completed, by means of which a dram and half of oxide of nickel was separated, which did not appear to contain any more oxide of cobalt.

Gives in odour
of oximuriatic
acid.

2. As the separation was not effected very well nor with much facility by the former method, the effect of sulphuric acid was tried. For this purpose, an equal quantity of water was poured on the oxide obtained as before, and sulphuric acid added till all was dissolved by the aid of heat. It then evidently gave out an odour similar to that of oximuriatic acid, although there was not any muriatic acid used. A like phenomenon, on a similar occasion, was before observed by the author (which is mentioned in the first section, page 18, of *Deitragé zur erweiterung*, for 1799.) The solution was then treated with ammonia as before, until the whole was almost dissolved. The residue, which was oxide of cobalt with a little oxide of nickel, had the colour of verdigris. When the solution was evaporated at a gradual fire, and separated by filtration from the precipitate, of which the greatest part was oxide of cobalt, it was submitted to spontaneous evaporation: It then crystallized without any farther separation, partly into prismatic crystals in groups, and of a green colour, and partly into crust, united together, and blue at the edges.

Treated with
ammonia depo-
sits oxide of
cobalt.

The solution
crystallized.

The crystals con-
tain cobalt and
nickel.

the essay of the oxides procured by potash from the solution of the crystals, as well as from the mother water, shewed that they contained cobalt almost in equal proportions,

The last expe-
riment repeated
on a larger scale

3. Mr. Baclol repeated the former experiments on a larger scale, in hope to obtain a better crystallization, and operated on eight ounces of cobalt ore, from which the first crystals, of a bluish green, obtained by a process similar to that last recited, and which weighed about five ounces, were again dissolved in 32 ounces of boiling water: This solution was evapo-
rated

perated till a pellicle was formed, and, after being filtered, was left near a fire, that it might cool slowly and crystallize. At the end of 48 hours, the greatest part of the salt was crystallized in beautiful tetrahedral rhomboidal pyramids, short, and of a yellow green, of which the lateral faces formed an angle of 15 and of 66 degrees, often with one extremity truncated; and always with an angle of 132 degrees towards its terminating face. This result proves that this salt forms more readily into regular crystals by cooling than by slow evaporation. All the crystals were then collected, washed with water, and again dissolved, and the nickel separated by boiling the solution with potash till the ammonia was disengaged.

Produces fine crystals in tetrahedral rhomboidal pyramids.

The crystals dissolved and the nickel separated as before.

4. As well to free this oxide from carbonic acid as to judge if it had been purified from cobalt, it was dissolved in nitric acid and treated with pure ammonia in the same manner as has been described. The liquor of a fine blue colour, (and from which a residue of five grains, which seemed to be an oxide of cobalt, had been separated by filtration), was evaporated to dryness. After another solution then made, it deposited an oxide of a beautiful bright green, which, after being washed and dried, weighed half an ounce. The liquor, which passed the filter, was analysed by pure carbonate of potash at the heat of boiling water, which then produced 170 grains of oxide of nickel, of a pale green, united to carbonic acid; a little of it was dissolved in muriatic acid, and some of the solution spread upon paper. On heating it afterwards, the tint became yellow, and inclined but very little to a green. But the oxide of nickel, which separated spontaneously during the evaporation, was dissolved in disengaging much oxymuriatic acid; spread on paper, it exhibited the colour, when heated, of a sympathetic ink of cobalt highly saturated; from whence it follows that it was more rich in cobalt than that produced from the precipitation.

The oxide obtained dissolved in nitric acid and treated with a narrow evaporated and it dissolved deposits a green oxide.

The filtered liquor yields by potash 170 grs. oxide of nickel.

Which contains less cobalt than the spontaneous precipitate.

The oxides dissolve in nitric and sulphuric acids.

The oxides collected in those two ways, dissolved in nitric and sulphuric acids, after becoming grey, (which the author supposed to be occasioned by the nickel dissolving first, and at least the greatest part of the cobalt remaining to the last, but which opinion was not confirmed by other experiments made on this subject.) These oxides made lightly red in the fire, changed their colour to a dark grey, and then, as well as by sulphuric acid.

Give out nitrous acid by fire, and by sulphuric acid.

on the addition of sulphuric acid, a disengagement of nitrous acid took place from the residue obtained by evaporation, which was also caused by the addition of an alkaline lixivium: With ammonia the same effects were produced which have been before mentioned.

Sulphites and nitrates of ammoniacal nickel always contain cobalt.

The results of the foregoing experiments are:—The phosphates and nitrates of ammoniacal nickel separated from cobalt ore, retain always some cobalt in their composition, and it is impossible from the method of Hermstadt modified in the preceding manner, to obtain an oxide of nickel without a mixture of cobalt.

The oxide of nickel remaining in the salt after evaporation contains very little cobalt.

b. By partially decomposing the ammoniacal nitrate of cobalt by evaporation, an oxide of nickel is obtained, very rich in cobalt, which contains nitric acid; and the oxide of nickel which remains undecomposed in this salt, retains a very small quantity of cobalt.

Dr. Schnaubert's method of obtaining pure oxide of nickel.

B. Doctor Schnaubert has published (in Tromsdorf's Journal of Pharmacy, vol. II. p. 66) a method of obtaining the oxide of nickel pure: Which consists in dissolving the metal of nickel mixed with cobalt, or its oxide separated from other substances, in nitric acid, in precipitating it by the carbonate of potash, and in heating it to a white heat, after washing and drying it. In this manner he always procured a yellow oxide, on which he caused very strong sulphuric acid to boil; which gave him a solution of oxide of nickel of a grass green, while the oxide of cobalt appeared in the form of a yellow residue. He proves the purity of the sulphate of nickel prepared in this manner, by the property which ammonia has of precipitating it of a bright green, and when added to excess, of re-dissolving it with a beautiful deep blue colour; but this argument appears insufficient to those who know that oxide of nickel, although mixed with many hundredth parts of cobalt, does not, however, experience any perceptible change in the colour of its precipitates, nor in its ammoniacal solutions. Besides the omission of indicating the means by which he was convinced that the oxide, which was the residue of the sulphuric acid solution, was really an oxide of cobalt, with the vague precept of heating the oxide acquired, without the least direction relative to the degree of the fire, and the uncertainty which he leaves of the degree of strength of the sulphuric

His test of its purity defective.

He has not mentioned his proof that the other oxide obtained was cobalt, —nor the degree of heat to be used, —nor the strength of the sulphuric acid employed.

fulphuric acid which he used, altogether throw doubts on the exactness of the process indicated, which the following experiments may elucidate.

1. A portion of the carbonic oxide of nickel, A 4, was exposed during an hour in a strong fire to a red heat approaching white heat. The oxide while hot was of a brownish yellow; after cooling it assumed a grey colour inclining to yellow, but not yellow. The oxide obtained by the evaporation A 4, having been treated in the same manner was still a little more grey than the preceding. The carbonic oxide of nickel was placed again for half an hour in a white heat; while hot it was yellow inclining to brown, but when cool, it was grey inclining to brownish yellow.

Experiment on
Dr. Schnaube's
process.

2. Thirty grains of this oxide made red (hot), were put for some hours to digest, with ninety grains of pure sulphuric acid of the specific gravity 1,960. Being then heated, the mass immediately swelled up with an explosive noise, and exhibited a yellow substance inclining to a green; by means of ebullition with half an ounce of water it was dissolved, except about a grain of a yellowish-grey powder, which proved to be an oxide of nickel mixed with cobalt and a little dirt. Thirty-five grains of oxide of nickel, (obtained by heating briskly to redness 60 grains of ammonical nitrate of nickel prepared by evaporation), afforded the same result, and the same phenomena, on being treated in the same manner: The same oxide being heated for half an hour to whiteness, using the bellows at the same time, did not afford a yellow mass, but one of a yellowish grey inclining to a green, which had the same effect with sulphuric acid that has been already related.

3. The experiment was again repeated with diluted sulphuric acid; 160 grains of ammonical oxide of nickel, which had been precipitated from many solutions were exposed for half an hour to the most violent white heat, under the operation of the bellows, after which they weighed 75 grains. This substance was of a greenish yellow here and there, and of a blueish grey where it touched the crucible; being broken it produced a black grey powder. It was mixed with a dram of sulphuric acid diluted with five drams of water; at that instant there was a rapid disengagement of gas, and on heating the mixture it evidently gave out hydrogen gas. After a suffi-

The experiment
repeated with
diluted sulphu-
ric acid.

cient ebullition, water was added, and the solution decanted off clear. The residue was treated again with weak sulphuric acid, and then gave a residue of ten grains which was by no means oxide of cobalt, but oxide of nickel mixed with cobalt, as its solutions in the acids and in ammonia proved. The two preceding solutions were each separately analysed by pure potash, and the precipitate was besides heated with an excess of potash, and then washed and dried. At the proof each of the precipitates afforded cobalt, which was always most pure in that of the first solution; for the solution in muriatic acid, laid on paper, and heated, inclined perceptibly to a yellow, while the precipitate of the second solution produced a stain of a clear and pure green. It is strange that the first solution afforded more oximuriatic acid than the second.

These experiments, and others made by the author; but not related, prove,

A. That the oxide of nickel heated either slightly or violently does not assume a yellow colour; and if this colour was observed by M. Schnaubert, it must have been caused by some substances which entered into the composition of the oxide, or perhaps by the mixture of a little arsenic.

B. That it is impossible by M. Schnaubert's method, to obtain an oxide of nickel exempt from cobalt; since it does not even effect a separation of the two oxides so far as to be perceptible to the eye.

C. M. Bucholz hints here at several experiments he made with a view to find an acid which would form an insoluble salt with one of the oxides, and one easy of solution with the other, but which, as they did not succeed, he does not mention; and as the method proposed by Mr. Lehman (in the *Cadmilogia*, part II, page 110) of fusing fifteen or twenty times, to a commencement of vitrification, a mixture of nickel and cobalt, in order to scorify all the cobalt, would be too troublesome and expensive, as would that also indicated by Bergman (*Opuscul, Physic, et chem*, Vol. II. p. 246—249) of repeating the fusion three or four times with from 8 to 12 times the quantity of pure nitre. The process indicated A 4, (consisting of a partial decomposition of the ammonical nitrate of nickel), alone remained to be repeated. For this purpose

The oxide of nickel mixed with oxide of

Each of the precipitates of the foregoing solutions afforded cobalt.

The experiments prove that nickel oxide does not become yellow; cause of the mistake of Dr. Schnaubert on this point.

It is not possible to obtain pure nickel in this way.

M. Lehman's method too troublesome and expensive, and M. Bucholz's allies

the process A 4 repeated.

oxide

oxide of nickel, (which was separated from the triple salt, not dissolved at the first evaporation, by carbonate of potash, was treated repeatedly, in such a manner) that after dissolving it in nitric acid, recourse, was had to the use of ammonia and evaporation as before described. In this method was obtained, entirely free from cobalt, an oxide separated by potash from the triple salt, which had been redissolved after evaporation, and which oxide had the properties mentioned in the memoir printed in the second volume of the *Annales de Chimie*.

The oxide which was separated by evaporation from the ammoniacal nitrate of nickel, was in the last operation entirely freed from cobalt; it only contained a still, as has been observed, a little nitric acid. The oxide of nickel, which, after having been laid bare by evaporation, still contains cobalt, may naturally undergo the same operation over again.

This method may be made use of until one more expeditions is discovered by farther experiments, since it does not occasion any considerable expence, for by potash, the evaporation of the ammoniacal nitrate of nickel may be effected in a retort, and also the subsequent decomposition of the triple salt, and thus the ammonia may be separated for other uses; in like manner, in works on a great scale, a part of the nitre may be recovered from the last operation, by the evaporation of the water in which the substance has been washed.

The oxide entirely freed from cobalt in the last operation.

This method recommended for the present, the ammonia may be saved during the process.

and the nitre recovered.

XVIII

*Sugar prepared from Beets. By M. HERBSTADT.**

THE method of M. Achard for extracting sugar from beets, was so expensive, that it was of no advantage for common use. M. Herbstadt, of Berlin, has practised another method, which is easily performed, and affords hopes of rendering this sugar cheaper than that from the sugar-cane; which is as follows:

After having bruised the beets in a mortar, M. Herbstadt submits them to the operation of a press, to extract the juice from them; which is then placed in vessels, and clarified with lime in the same manner as cane-sugar.

The expressed juice of the beet is, clarified by lime,

and then evaporated to a syrup. A coarse sugar is produced on cooling.

When this operation is finished, the liquor is evaporated to the consistence of a syrup: It is then left to cool, and a coarse sugar is obtained, of a dark-brown colour. At the bottom of the vessel a syrup remains, which may be used for domestic purposes.

From 100 lb. of this coarse sugar, 50 lb. of refined sugar may be obtained.

From 100 pounds of the coarse sugar, eighty pounds of well crystallized sugar are obtained by the first refining, which sugar is not at all inferior in quality or whiteness to that of the cane. The whole operation may be completed in two days.

It is probable M. Hermbstadt used the common field beet, or root of scarcity.

The particular species of beet which M. Hermbstadt used in his experiments, is not mentioned; but it is most probable that this chemist made use of the common field beet, known in Germany by the name of *mangel wortzel*, the culture of which is spread through many cantons of Germany. This variety, however, contains less sugar than all the other species of beets; and, for this reason, M. Sonnini is of opinion, that if sugar can be obtained from beets with profit and economy, more success would be obtained by submitting to the operations described, the small red beet, called in France that of *Castlenaudery*, which is the sweetest of all.

A better produce might be obtained from the small red beet of Castlenaudery.

XIX.

Method of stacking Turnips, to preserve them through the Winter.

By Mr. JOHN SHIRKEFF, of Captain Head, near Had-
dington, N. Britain.*

Rapa solo molli et acre humidulo lætantur.

Preservation of turnips through the winter.

SATISFIED, from observation and experience, that turnips are the foundation of the best husbandry on almost all soils and situations in the arable districts of Great Britain; and that this crop should always be drawn, except from blowing sands, or light moorish soil, on both of which it should always be in part consumed on the ground with sheep; convinced also, that turnips, if possible, should be off all soils, and the land

* See Arts, 1805. The premium of 30 guineas was awarded for this method.

ploughed

ploughed up before the middle of December, at the latest, to secure the succeeding corn crop, and grasses, or clovers, with either of which every field that carried a turnip crop the preceding season, should, in almost every case, be sown down; and impressed with the many high advantages attending this practice, as soon as my pea and bean stubbles are ploughed up, and sown with wheat, my turnips are begun to be drawn, and stacked up for use during the following winter and spring. If the distance of the turnip-field from the homestead does not exceed a quarter of a mile, two double horse carts only are employed, and more in proportion to the distance of the turnip field, or number of hands you may be able to command to carry on the work. One clever driver is sufficient for two carts, and two for three carts, &c. one cart being always in the field loading or loaded. On being brought home, the turnips are instantly tumbled out at the stack; which is done with great facility, from the construction of the carts in this district, which to convenience and strength likewise add lightness, to enable horses to move at a smart pace with them when empty. The turnips tumbled out of the cart, are trimmed of their leaves, and cleaned of any earth that may adhere to them, by women, &c. before being put into the stack. Old table-knives do very well for the purpose, and the leaves should be cut off close to the root; the back of the knife being used for removing any pieces of soil that may stick on the turnip.

Women, &c. trim the turnips, and put them into strong coarse wicker baskets, to be carried forward by a man, who hands them to another, who lays them into or on the stack. The ground on which the turnips are placed ought to be dry bottomed. If that is not the sort of soil where you find it most convenient to make your stack, a quantity of boulders may be put on, regularly spread over the space, to the thickness of at least eighteen inches. My corn-rick yard, being dry ground, has been used as the place for keeping my turnips in. The stacks have been made about ten feet wide, by driving a row of stakes into the ground parallel to the wall of the yard, which serves instead of another row. The wall is only about five feet and a half high, and the stakes are driven to the same height. The inside of the wall and stakes are lined with compact bunches, or sheaves of wheat.

Preservation of
turnips through
the winter.

straw, about ten inches in diameter, placed horizontally on the ground or boulders, and introduced, as wanted, during the operation of stacking. A fire of the largest turnips are placed one above another, on the inside of the bundles of straw, more particularly on the side guarded by the stakes, till the pile reaches the height of five feet from the ground, or from the boulders; if it has been found necessary to spread any over the ground. The inner part of the stack is at the same time gradually made up with turnips put in promiscuously; along which a plank is laid, and occasionally shifted as the pile rises, for the man who builds the stack to stand on without bruising the turnips with his shoes. When the pile of turnips is reared, in the manner described, to the height of above five feet, it is gradually contracted inwards, on both sides, at an angle of about forty-five degrees, like the roof of a barn; the largest turnips being still piled on the outside, till the roof is so far completed. The stack is every day so far finished in height as it is extended in length, and is covered with wheat straw thatch, roped down with twisted bands of oat-straw before evening, to secure the stacked turnips from rain that may fall during the night. The thatch is laid on a foot thick, and secured in the same simple, effectual manner, that corn-ricks are covered in Northumberland, Berwickshire, and the Lothians; with this difference only, that the straw is four times as thick laid on the turnip as on the corn, to exclude cold as well as wet; and that there is a rail of wood stretched, hanging horizontally at the tops of the wall and stakes, to fix the straw ropes to, which secure the thatch on the stack. The end of the stack is every night covered with bundles of wheat-straw, which are removed next day, or when building recommences.

Three men are employed in the field to load and dispatch the carts, occasionally assisting four women who draw the turnips, striking off the top root with a strong heavy knife, leaving the turnips on the tops of the drills as drawn and chopped, with the leaves all in one direction, to be readily laid hold of by the men who lift them up to the cart. The horses pass along in the space between the two rows or drills of the turnips, which may be drawn: and, being at thirty inches apart, and the extremities of the wheels about five feet from each other, it is evident a wheel runs in the middle

of

of each space between the contiguous drills, without injuring the turnip, whether drawn or not. When the cart is about to turn, after being loaded, the men move the turnips to make room for the horses, putting them into the cart as part of the load.

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*Expences of drawing, carting, trimming, stacking, covering, &c.
a statute acre of good turnip,—at the distance of not more
than a quarter of a mile from the stack.*

	£.	s.	d.
Two double-horse carts, and one man	-	0	16 0
Two men loading, drawing, building, &c.	-	0	3 4
Seven women drawing and trimming	-	0	4 1
Two girls trimming	-	0	1 0
Four ditto and boys ditto	-	0	1 8
Twisting ropes, drawing thatch, thatching, waste of thatch, stakes, &c. say	-	0	3 6
		1	14 7

The above is a fair average of the expence of securing somewhat more than twelve and a quarter statute acres last season, which was all I drew; and one field of two acres, one rood, thirty-three perches, was so far distant as to require three carts, and two drivers. That field, however, was first drawn, and the weather being fine and moderate, more work was done in proportion to the length of the day, which was also longer. Women and children cannot, indeed, exert themselves with spirit, in raw cold weather. October is perhaps the best month to draw in. It is a question with me, whether the average of the acres that are under turnip in the island, if the weight exceeds twenty-four tons, does not cost more, merely for drawing and carting only. When it is considered that this operation is performed often in cold, frosty, and stormy weather, and that frequently much snow may be to be removed before the turnip can be seen. If no snow has fallen before the frost sets in, the turnips must be hoed up with instruments for the purpose. Many are cut, and much left in the ground of the lower part of the root. After all this labour, what is obtained is frequently no better than a lump of ice, environed with earth, frozen so firmly to its surface, that nothing but thawing in

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the winter.

cold water can ever render it fit to be touched by the mouth of any animal whatever.

Admitting, however, the expence of drawing and carting to be the same, all that can be stated as extraordinary expence is the cost of trimming and stacking, which amounts to 11s. 3d. an acre. On the other hand, we have the advantage of having fine fresh clean turnips, always secure and at command, to carry on feeding and breeding stock; at the same time that all loss by rotting in the spring months is prevented, which is frequently thirty and even fifty per cent. on all the crop that remains in the field, after the first of February. Above all, the practice of drawing and stacking before winter, by admitting of early ploughing to mellow the soil, secures a valuable corn, and succeeding clover crops. When all these circumstances are maturely weighed, the expence of eleven shillings and three-pence will, to every enlightened agriculturist, appear but trifling to obtain such very valuable advantages. The writer of this little essay has had the satisfaction of having excellent crops after his turnips, this season; while almost every other crop in the neighbourhood was indifferent; and some on rich dry loams, high rented, by being sown in the months of April and May, on the spring ploughing, after turnips eaten off with sheep, were so miserable, as evidently to pay *nothing* after expences of labour, seed, and reaping. The young clovers too, sown with these crops, have almost entirely perished from want of moisture. The loss of the crop and clover seed is not all: the system suffers a derangement, the consequences of which none but practical men can calculate.

One thing remains to be noticed, which is, that twenty-six young cattle, cows, and yearling calves, were kept nearly three weeks on the turnip trimmings, with oat-straw along with them, to their improvement; and that many more might have been kept, had they been provided in time. A quantity of good manure was made: and, estimating all advantages arising from the consumption of the leaves in this way, at no more than 3d. a head per night, for the keep of each beast, the amount will exceed the expence of trimming and stacking the whole crop of turnips on twelve acres and a quarter.—The leaves that remain on turnips after Christmas, are either unfit to be eaten, or wasted by the frosts.

T. SHIRKEFF.

XX.

*Account of some Specimens of Basaltæ from the northern Coast of Antrim. By the Rev. Dr. WILLIAM RICHARDSON.**

THE Reverend Dr. William Richardson, late F. T. C. D. ^{Remarks on the basaltæ of the coast of Antrim.} having sent to Dr. Hope a collection of specimens from the northern coast of Antrim, with a catalogue and observations, the specimens were exhibited, and the observations were read in the Royal Society, March 1803.

Siliceous Basalt.

Dr. Richardson discovered the fossil to which he gives this name, in the peninsula of Portrush, four or five years ago. It abounds also in the Skerry islands, a reef of rocky islets extending from the northern point of Portrush-head for about a mile eastward. A small part of every one of those islets is formed of this stone, while the remainder consists of coarse basalt, similar in all respects to that on the east side of the above-mentioned peninsula. It is met with in one or two other places.

This stone is arranged in strata, from ten to twenty inches thick, all steadily parallel to one another, and every stratum, as far as can be observed, preserving an uniform thickness through its whole extent. When these strata are quarried into, they appear to be constructed of large prisms, generally pentagonal, which when broken divide into smaller prisms. This internal prismatic construction frequently gives an irregular or shivery appearance to the fracture, which however is often conchoidal, and the grain as uniform as in the Giant's Causeway basaltæ.

The beds of this fossil are remarkable for containing marine exuvizæ in great abundance, particularly impressions of *cornua ammonis*. The flat shells and impressions contained in these stones, are steadily parallel to each other, and perpendicular to the axis of the prisms. It must be observed, that the prismatic construction is never interrupted by the shells dispersed through it; the planes which separate the prisms passing equally through the shells and the stone itself.

* Edinburgh Transf. Vol. V.

Remarks on the
basaltes of the
coast of An-

The grain of this stone passes by insensible shades from a high degree of fineness, until it become undistinguishable from that of the common columnar basaltes.

The name of Siliceous Basalt, which Dr. Richardson employs, was first given to this fossil by Mr. Pictet of Geneva, when he visited Portrush, in a tour through Ireland two years ago. He considered it as a variety of basalt, containing a greater proportion of *silica* than usual.

The strata of siliceous basalt, both at Portrush and the Skerry islands, generally alternate with strata of equal thickness of a coarse-grained basalt of a grey colour. The materials of the strata grow into each other, so as to form one solid mass, from which it is easy to quarry pieces in the confine of the two strata, with a part of each adhering; but the coarse basalt, as it approaches very near to the fine, always abates somewhat of its coarseness; yet the line of demarcation is left completely distinct.

(The conclusion in our next.)

SCIENTIFIC NEWS.

Almanack printed at Constantinople.

Almanack
printed at Con-
stantinople.

FOR the first time an almanack has been printed at Constantinople, under the direction of Abdorahman. The printing-office was established in 1716, by *Said* (who had been at Paris with his father, the ambassador), and by Ibrahim, an Hungarian: Achmet the Third patronized them, and they printed many books; but an almanack was never before printed.

Observatory at Bavaria.

Bavarian obser-
vatory.

The Elector of Bavaria, a few months before the arrival of the French armies, caused an observatory to be erected in the neighbourhood of Munich. The situation chosen for its construction, takes in an extensive horizon. Professor Seyfer, a celebrated astronomer of Gottingen, was nominated director of this establishment.

Establishments for Natural Philosophy in the Ukraine.

The rich land-owners in the Ukraine and Volhinia, have contributed largely for the establishment of Lyceums for teaching natural philosophy, at Krzemynico, and at Winnica. The library and philosophical apparatus of the King of Poland, have been purchased for this purpose. M. Sniadecki has received a sum equal to 500*l.* to purchase telescopes and clocks; and no expence is to be spared in properly furnishing the observatories with instruments.

Observatory at Moskow.

M. Goldbach, an able astronomer of Leipzig, has been nominated professor of the university of Moskow, with a salary equal to 250*l.* He is to have the direction of the construction of a new observatory, to furnish it with instruments, to make regular observations, and to instruct some young men in practical astronomy who have been previously instructed in the preparatory sciences, and to give a course of lectures in theoretical astronomy in one of the halls of the University.

They possess many of Cary's telescopes, of different powers; an excellent astronomical clock; a chronometer, made by Arnold; a portable circle, of one foot diameter; and, it was reported, had ordered one of three feet diameter from the successor of Mr. Ramsden: Thus M. Goldbach will be provided with every instrument necessary and useful to astronomy, at the observatory of Moskow.

M. Goldbach has taken the opportunity of his journey, to determine the position of some towns; among others that of Riga, $1^h 27'.0$, and $56^{\circ} 57'.9$.

At the same time that M. Goldbach is engaged with the astronomical establishment at Moskow, MM. Schubert and Wisniewski are employed at the observatory of Petersburg; and there is reason to expect a series of observations from that part of the world.

Solar Tables.

A set of tables of the sun, composed by M. Delambre, have been printed at Paris, in which there are many new equations,

tions, and of which all the elementary parts have been verified by new observations. A set of tables of the moon's motion are also to be printed, and when they are completed, those of the planets will follow.

Bequest of Ernest the Second relative to his Observatory.

Ernest the Second's bequest to his observatory.

Ernest the Second, late Duke of Saxe-Gotha, was remarkably attached to astronomical studies. He made observations and calculations himself, assisted in composing books on the subject, and furnished the funds for their publication. He enabled M. Zach to measure a degree of the meridian in Germany, and defrayed the expences from his private purse; so that he united to the merit of a connoisseur in the science, that of an author, a patron, a man of science, and of a generous prince.

He left in his will a sum equal to about 1330*l.* to form a fund for the maintenance of the observatory of Seeberg, near Gotha, which was built out of his own private estate; and ordered his successor to erect no other monument to his fame, but the careful support of this establishment.

Baron de Zach, who has given a copy of the will in his Journal, adds, "That he can assure the lovers of science, that the will of the father will not only be fulfilled, but surpassed by his successor, the present Duke Emilius Leopold Augustus, who has already shewn the most marked proofs, of his attachment to the sciences.

"In a codicil to the will the Duke repeated, 'I forbid expressly the elevation of any monument to my memory, or even an epitaph, or any monument at or near my tomb.'

A
JOURNAL
OF
NATURAL PHILOSOPHY, 'CHEMISTRY,
AND
THE ARTS.

APRIL, 1806.

ARTICLE I.

*Letter from T. YOUNG, M.D. F.R.S. &c. claiming the Lamp
described in our last Number, and demanding an Explanation
from the anonymous Communicator.*

To Mr. NICHOLSON.

SIR,

I WAS much surprized on seeing, a few days ago, the figure of a lamp contained in the fourth plate of your Number for February last. I trust you will be convinced, upon inspection of the figure which I now send you, and which was engraved before Christmas, that your correspondent A. F. must have copied his lamp from that which is here represented; and I am sure you will think I have a right to demand a public explanation of the manner in which he procured a sight of a plate not yet published, and of the motives which induced him to make so unjustifiable a use of it. I shall reserve the complete explanation of this lamp for the work to which the plate belongs, which has been long in the press, and which will soon be ready for publication; I shall only observe that

Concerning the
invention of a
Lamp.

it is in a great measure free from the inconvenience which A. F. has attributed to it, (p. 168) and that the "small shaded circle" is not a "perforation," but a weight attached to the counterpoise,

I am, Sir,

Your very obedient Servant,

THOMAS YOUNG.

Wellbec Street,
March 15, 1806.

II.

On the Tendency of Elastic Fluids to Diffusion through each other. By JOHN DALTON*.

Mixed elastic fluids of different densities do not separate;

IN an early period of pneumatic chemistry it was discovered that elastic fluids of different specific gravities being once diffused through each other, do not of themselves separate, by long standing, in such manner as that the heaviest is found in the lowest place; but on the contrary, remain in a state of uniform and equal diffusion.

but will they mix without agitation.

Dr. Priestley has given us a section on this subject (vid. Experiments and Observations, &c. abridged. Vol. II. p. 441) in which he has proved the fact above-mentioned in a satisfactory manner; and every one's experience since, as far as I know, has coincided with his conclusions. He has not offered any conjecture concerning the cause of this deviation from the law observed by inelastic fluids; but he suggests that "if two kinds of air of very different specific gravities, were put into the same vessel, with very great care, without the least agitation that might mix or blend them together, they might continue separate, as with the same care wine and water may be made to do."

Dr. Priestley thinks not.

The determination of this point, which seems at first view but a trivial one, is of considerable importance; as from it we may obtain a striking trait, either of the agreement or disagreement of elastic and inelastic fluids in their mutual action on each other.

* Manchester Memoirs, Vol. I. New Series,

It is, therefore, the subject of the following experiments to ascertain whether two elastic fluids brought into contact, could intermix with each other, independently of agitation. Inquiry by experiment which shews the contrary.

The result seems to give it in the affirmative beyond a doubt, contrary to the suggestion of Dr. Priestley; and establishes this remarkable fact, that a lighter elastic fluid cannot rest upon a heavier, as is the case with liquids; but, they are constantly active in diffusing themselves through each other till an equilibrium is effected, and that without any regard to their specific gravity, except so far as it accelerates or retards the effect, according to circumstances.

The only apparatus found necessary was a few phials, and tubes with perforated corks; the tube mostly used was one ten inches long, and of $\frac{1}{8}$ inch bore; in some cases a tube of 30 inches in length and $\frac{1}{4}$ inch bore was used; the phials held the gases that were subjects of experiment and the tube formed the connection. In all cases, the heavier gas was in the under phial, and the two were placed in a perpendicular position, and suffered to remain so during the experiment in a state of rest; thus circumstanced it is evident that the effect of agitation was sufficiently guarded against; for, a tube almost capillary and ten inches long, could not be instrumental in propagating an intermixture from a momentary commotion at the commencement of each experiment. Apparatus.

FIRST CLASS.

Carbonic Acid Gas, with Atmospheric Air, Hydrogerous, Azotic and Nitrous Gases.

1. A pint phial filled with carbonic acid gas, the 30 inch tube and an ounce phial, the tube and small vial being filled with common air, were used at first. Carbonic acid gas with lighter gases. In one hour the small phial was removed, and had acquired no sensible quantity of acid gas, as appeared from agitating lime water in it. In three hours it had the acid gas in great plenty, instantly making lime water milky. After this it was repeatedly removed in the space of half an hour, and never failed to exhibit signs of the acid gas. Things remaining just the same, the upper phial was filled with the different gases mentioned above repeatedly, and in half an hour there was always found acid sufficient to make the phial $\frac{1}{2}$ filled with lime water quite

milky. There was not any perceptible difference whatever gas was in the upper phial *.

SECOND CLASS.

Hydrogenous Gas with Atmospheric Air and Oxygenous Gas.

Hydrogen, with
atmospheric air
and oxygen.

1. Two six ounce phials were connected by the tube of a tobacco pipe, three inches long, the upper containing hydrogenous gas, the lower atmospheric air: after standing two hours, the lower phial was examined; the mixed gases it contained made six explosions in a small phial. The gas in the upper also exploded.

2. Two four ounce phials connected with the ten inch small tube stood two days, having common air and hydrogen gas. Upon examination the upper was found to be $\frac{1}{2}$ common air by the test of nitrous gas. The gas in the under exploded smartly; that in the upper moderately with a lambent flame.

3. Two one ounce phials were connected by the ten inch tube, containing common air and hydrogenous gas; in three hours and a half the upper was about $\frac{1}{2}$ common air and the under $\frac{2}{3}$; the former exploded faintly; the latter smartly.

4. Two one ounce phials were connected as above; the under containing gas about $\frac{3}{4}$ oxygenous, the upper hydrogenous: In three hours the latter was $\frac{1}{2}$ oxygenous, and the former about $\frac{1}{2}$; the upper exploded violently, the under, moderately.

5. Two one ounce phials were again connected, the lower having atmospheric air, the upper hydrogenous gas; they stood fifteen hours, and were then examined; the upper gave 1.67 with nitrous gas, the under 1.66.—Hence it is evident that an equilibrium had taken place, or the two gases were uniformly diffused through each other in both phials.

THIRD CLASS.

Nitrous Gas, with Oxygenous Gas, Atmospheric Air, Hydrogenous and Azotic Gases.

Nitrous gas
with oxygenous,
atmospheric hy-
drogenous, and
azotic.

The results of the preceding experiments upon gases that have no known affinity for each other, were conformable to

* The small tube of ten inches was then used and a phial of common air; in one hour much acid gas had come through, as appeared by lime water.

what

what *à priori*, I had conceived; for, according to my hypothesis, every gas diffuses itself equably through any given space that may be assigned to it, and no other gas being in its way can *prevent*, though it may considerably *retard* this diffusion. But in some of the following experiments, in which the two gases are known to have a chemical affinity for each other, I expected different results from what are found; perhaps without sufficient reason. For, chemical union cannot take place till the particles are brought into contiguity; and the elastic force which sets them in motion appears, from the above experiments, to be a principle diametrically opposite to affinity. That circulation of elastic fluids, therefore, which we have now before us, cannot be *accelerated* by their having a chemical affinity for each other. Another circumstance deserves explanation;—when nitrous and oxygenous gas are in the two phials, the residuary gases after the experiment are nearly as pure as before; because those portions of them that meet in the tube, form nitrous acid vapour, which is absorbed by the moisture in the phials, and therefore does not contaminate either gas.

Nitrous gas
with oxygenous,
atmospheric hy-
drogenous, and
azote.

1. Two one ounce phials were connected with the small tube, the under containing nitrous gas, the upper atmospheric air; after three hours, the upper phial was taken off when a quantity of air was perceived to enter, as was expected; the air in the upper phial was scarcely distinguishable from what it was at first; that in the under phial was still so much nitrous as to require its own bulk of common air to saturate it.

2. The above experiment was repeated, and the upper phial drawn off when the whole was under water, in order to prevent communication with the atmosphere: about $\frac{1}{2}$ of an ounce of water entered the phials, to compensate the diminution. Remaining air in the upper phial was a very little worse than common air, it being of the standard 1,47 when the former was 1,44. The gas in the under phial was still nitrous and nearly of the same purity as at first; for three parts of it required four of atmospheric air to saturate them.

3. Nitrous gas and one $\frac{2}{3}$ oxygenous were tried in the same way: after four hours, the apparatus was taken down under water. The upper phial was $\frac{2}{3}$ filled with water, and the
gas

gas in it was partly driven down the tube into the other phial, by which, and the previous process, the nitrous gas was completely saturated and nothing but azotic with a small portion of oxygenous were found in the under phial: the remaining gas in the upper phial was still $\frac{1}{2}$ oxygenous.

4. Nitrous gas and hydrogenous: in three hours the upper phial was $\frac{1}{2}$ nitrous, and of course the under must have a like part of hydrogen.

5. Nitrous gas and azotic: after three hours the upper phial was $\frac{2}{3}$ nitrous.

In the two last experiments, the quantity of nitrous gas in the upper phial was less than might be expected; but the tube was at first filled with common air, and some must enter on connecting the apparatus, which is sufficient to account for the results.

FOURTH CLASS.

Azotic Gas, with Mixtures containing Oxygenous Gas.

Azote with
oxygenous com-
pounds.

1. Azotic gas and one $\frac{2}{3}$ oxygenous: after standing three hours the upper phial was of the standard 1.78, or about $\frac{1}{14}$ oxygenous.

2. Azotic gas with atmospheric air: after standing three hours: the upper phial was not sensibly diminished by nitrous gas; the under phial, however, had lost two per cent, or $\frac{1}{50}$ of its oxygen. The reason of this was, that the azotic gas in this experiment having been just made for it from nitrous gas, this last had not been completely saturated with atmospheric air, and hence had seized upon all the oxygen ascending into the upper phial.

Having now related all the experiments I made of any importance to the subject, it will be proper to add, for the sake of those that may wish to repeat some of them, that great care must be taken to keep the inside of the tube dry; for if a drop of water interpose between the two gases, I have found that it effectually prevents the intercourse: glass tubes should therefore be used, that one may be satisfied on this head, as the obstruction will then be visible.

I shall make no further comments on the above experiments, by way of explanation: because to those who understand my hypothesis of elastic fluids, they need none: and I think it would be in vain to attempt an explanation any other way.

I can

I cannot however, on this occasion, avoid adverting to some experiments of Dr. Priestley, which few modern philosophers can be unacquainted with: I mean those relating to the seeming conversion of water into air. (Vid. *Philos. Transact.* vol. 73, page 414,—or his *Expts.* abridged, vol. 2, page 407.) He found that unglazed earthen retorts containing a little moisture, when heated, admitted the external air to pass through their pores at the same time that aqueous vapour passed through the pores the contrary way or outward; and that this last circumstance was necessary to the air's entrance. The retorts are air-tight, so far as that blowing into them discovers no pores; but when subjected to a greater pressure, as that of the atmosphere, or even one much short of it, they are not able to prevent the passage of elastic fluids. The fact of air passing into the retort through its pores, and vapour out of them at the same time, are elegantly and most convincingly shewn by Dr. Priestley's experiments, in which he used the apparatus represented in plate 7, fig. 1, of the edition above referred to. The Doctor confesses his explanation of these remarkable facts is very inadequate; and no wonder, for it is impossible for him or any other to explain them on the commonly received principles of elastic fluids. But we will hear what he says on the subject:—"At present it is my opinion, that the agent in this case is that principle which we call *attraction of cohesion*, or that power by which water is raised in capillary tubes. But in what manner it acts in this case I am far from being able to explain. Much less can I imagine how *air* should pass one way and *vapour* the other, in the same pores, and how the transmission of the one should be necessary to the transmission of the other.—I am satisfied, however, that it is by means of such pores as air may be forced through, that this curious process is performed; because the experiment never succeeds but in such vessels as, by the air-pump at least, appear to be porous, though in all such."

The remarkable experiment of Priestley, of air entering earthen retorts while water passed out in a vapor.

Dr. Priestley's explanation or conjectures.

The truth is, these facts so difficult to explain are exactly similar to those which are the subject of this memoir: only instead of a great number of pores we have one of sensible magnitude, (the bore of the tube.) Let the porous retort have the same elastic fluid within and without, in the one case; and the two phials contain the same elastic fluid in the other, then

The fact is, that the steam and air mix by means of the pores.

—and the same
happens in any
two gases.

then no transmission is observable in either; but if the retort have common air, or any other gas, without, and aqueous vapour, or any other elastic fluid, except the outside one, within; then the motion in and out commences, just as with the phials in similar circumstances. In fact this last observation has since been verified by Dr. Priestley himself, of which an account is given in No. 2, of the American Philosophical Transactions, vol. 5. After alluding to his experiments above-mentioned, he observes, “ Since that time I have extended and diversified the experiments, and have observed, that what was done by *air* and *water*, will be done by *any two kinds of air*, and whether they have affinity to one another or not, that this takes place in circumstances of which I was not at all apprized before, and such as experimenters ought to be acquainted with, in order to prevent mistakes of considerable consequence.”

The facts stated above, taken altogether, appear to me to form as decisive evidence for that of elastic fluids which I maintain, and *against* the one commonly received, as any physical principle which has ever been deemed a subject of dispute, can adduce.

III.

*On the Horizontal Moon. By Dr. OKELY. In a Letter from
Mr. H. STEINHAEVER.*

To Mr. NICHOLSON.

SIR,

Fulnuk, March 1, 1806.

ABOUT the beginning of last year, I had the pleasure, in compliance with your obliging letter to send you impressions of the Egyptian Scarabacus, which I hope came safe to hand. Your kindness in inserting my trivial remarks upon the same in your valuable Journal, encourage me to submit the following short essay, upon a subject which has employed the ingenuity of several of your correspondents, which I received from my friend Dr. Okely, of Wyke, near Hallifax, in consequence of some conversations occasioned by the perusal of your work.

If you think it worthy a place in your collection, it will be considered as an additional obligation conferred on,

SIR,

Your obedient servant,

H. STEINHAEUER.

Observations on the seemingly enlarged apparent Diameters of the Sun and Moon, when viewed in or near the Horizon.

Every one who views the sun or moon, when they are in the horizon, thinks that they appear larger than when they are seen in any more elevated part of the heavens. And astronomers know, that the distance of the same fixed stars is apparently greater when seen near the horizon, than when they are more elevated. But it is likewise well known to astronomers, that the apparent magnitudes of the sun and moon, as well as the apparent distances of any given fixed stars, as measured by the micrometer, are the same in that part of the heavens which is near the horizon, as, in the same circumstances, they are found to be in any other part, except that the moon, being really perceptibly farther from an observer, placed on the earth's surface, when she appears in the horizon, than when she appears in the zenith, is found to have a smaller apparent magnitude, agreeing with the causes to which it is known to be owing. The first mentioned phenomena must therefore belong to the head of *optical deceptions*. Let us enquire from what source this deception arises.

General fact stated that the heavenly bodies seem larger at low altitudes.

I am not the first by whom the source was sought for in the apparent flatness of the sky; but I differ as far as I know, from all others in my manner of connecting one appearance with the other.

In order to explain my idea of the matter, I shall first attempt to show that the flattened appearance of the visible heavens is not an illusion, but a reality; or in other words, that an observer placed on the earth is really at a greater distance from a point of the sky, situate in the horizon, than from a point situate in the zenith.

Explanation of the flattened appearance of the heavens.

This will appear in the clearest manner if we endeavour to give an answer to the two following questions: *What is the sky?* and *Where is the sky?*

The sky is a real object, and offers a flattened concavity.

By the sky, I mean that blue concave superficies, within which every observer on the surface of the earth finds himself placed

placed

placed. What is this? It is certainly something real and material, or else it would not appear coloured. For bodies, to appear coloured, must have parts of some determinate magnitude.

Where doth it exist? Not in those immensely distant parts of space, where the heavenly bodies revolve. For if those spaces contained any bodies of a determinate magnitude, and consequently of a determinate density, the heavenly bodies could not continue through ages to revolve in the same periodic times; their momentum would be diminished by resistance, and the periodic times of their revolution would change. The blue sky therefore cannot be placed beyond the atmosphere of our earth. The smallest parts of bodies, that are coloured are blue, and the blue sky is therefore either the atmosphere itself or the smallest and most elevated vapours ascending in it, or both together. The heavenly bodies shine through it, and therefore it cannot be opaque; it is itself of a blue colour, and therefore is not perfectly transparent.

Though we are ignorant of the exact height of the atmosphere, yet we may take it for granted, that it does not extend as far as the moon, and therefore that the distance of its farthest points from the centre of the earth has a finite ratio to the semi-diameter of the earth. That the ratio is probably less than 2 : 1.

If therefore the blue heavens which surround the earth, and are concentric with it, have a semidiameter not double that of the earth, their horizontal points as viewed from the earth, must be farther from us than any that are nearer the zenith.

For let $A'CF$ (Plate VII. Fig. 1.) represent a great circle of the earth, and AC be its radius, and let the circle $DBEG$ represent a great circle of the atmosphere drawn with a radius AB not $= 2. AC$, the line $CD > BC$; $HC > BC$.

—which is not
always alike.

I was led to this solution of the flattened appearance of the heavens, by observing that, when the sky is uniformly overcast with clouds, the concave superficies appears considerably flatter than when the sky is serene. In the former case, the two concentric circles in the figure approach nearer to each other, the clouds being nearer to the earth than the sky is, and the ratio of DE to CB must of course increase.

Explanation
from a diagram
of the sky, that

But to proceed. When any bodies situated behind a semi-transparent screen are seen through it, they will appear to be fixed

fixed in the screen at the points of intersection, which lines drawn from every point of the bodies to the eye of the observer, make with the screen. Now such a semi-transparent screen, the blue skies, interpolate between the heavenly bodies and our eyes. They will therefore appear to be fixed in the sky, at the above-mentioned points of intersection.

But if lines DC , HC , IC , BC , be drawn so that the angle at C are equal, they may be considered as coming from the extreme points of bodies which subtend equal angles of vision, or which have the same apparent magnitude. The angle DCH may be considered as representing the angle of vision which the sun subtends at the horizon. The equal angle HCI , the angle subtended by the same body in a more elevated situation. But $DH > HI$. Thus the heavenly bodies must appear enlarged in their vertical diameters, when in the horizon; and the same may be shewn of any other diameter. They will therefore appear uniformly enlarged; which was the thing to be explained.

W. OKELEY.

IV.

Account of some Specimens of Basalttes from the northern Coast of Antrim. By the Rev. Dr. WILLIAM RICHARDSON.

(Concluded from Page 273.)

THE peninsula of Portrush lies about six miles to the west of the Giant's Causeway, and on its eastern surface alone presents these strata. Remark on the basalttes of the coast of Antrim.

In the space of about 700 yards, it exhibits in miniature those changes and interruptions of the strata, which occur on the large scale along the northern basaltic coast of Ireland. At the place where it emerges from the strand, there first occurs a mass composed of strata of the coarse and siliceous basalt, placed over each other alternately; this is succeeded by an accumulation of regular strata of the coarse basalt alone. A second alternation, and a second accumulation of the coarse-grained strata, come in order, and extend to the well called Tubber Wherry. Here commences an accumulation of many strata of the siliceous

Remarks on the basalt alone, which stretches along the shore for about 100 yards; and then changes into a third alternation, which continues to the little boat-harbour, called *Port-in-too*, near which the siliceous basalt disappears. Over this stretch, notwithstanding the frequent change in the arrangement of the strata, the thickness of each stratum, of both species, remains pretty nearly the same, and the position of them all steadily so, viz. with a considerable dip to E. N. E.

The west side of the peninsula, though only about 400 yards distant, consists entirely of coarse basalt. It shows a bolder face, and is formed of rude massive pillars, from 60 to 80 feet long.

"I am aware," says Dr. Richardson, "that several mineralogists deny the shell-bearing stone to be basalt, while others contend strenuously that it is. I will not venture to decide on the question, but must remark, that I have never met with it but contiguous to basalt, and so solidly united to this last, that the continuity of the whole mass was uninterrupted. The grain of the stone graduates, as has been already remarked, into that of the common basalt; and the arrangement of it and that of the basalt, with which it is so much mixed at Portrush and the Skerry island, is exactly the same; the strata of each scarcely differing in thickness, and not at all in inclination. The strata of both kinds break into prisms, and the surfaces, where accessible, exhibit the appearance of causeways, differing only in this, that in the siliceous basalt, the pentagon is the prevalent figure, and in the coarse basalt, the quadrangle. The fusibility of both stones is also nearly the same; the shells in the siliceous basalt are calcined in the fire, and many more are then discovered which had before escaped the eye *."

Whinstone

* Dr. Richardson observes, that some mineralogists deny that this fossil is basalt. Several of the members present when this paper was read, some of whom had examined the stone in its native place, were of that number. It was remarked, that though certain portions of the strata of this fossil bore much resemblance to some species of basalt, by far the greater part of the mass bore no resemblance whatever to any.

It was also stated, that the substance of the coarse-grained, undisputed basalt, which lies between the strata of this stone, does not contain any vestiges of marine animals; That veins often issue from

Whinstone Dikes on the Coast of Antrim.

Remarks on the
basaltes of the
coast of Antrim.

Dr. Richardson describes some particulars in the construction of the whinstone dikes on the coast of Antrim, which appear singular, and deserving of attention. These dikes, he says, are uniformly formed of large massive prisms laid horizontally, which are always divisible into smaller prisms that are likewise horizontal. To prevent confusion, he calls the first of these *component prisms*, and the second, or smaller ones into which the others break, *constituent prisms*.

The component prisms are sometimes of enormous size, and in the same dike are nearly equal; the constituent prisms are small, (the sides about an inch long), and neatly formed.

The dike which traverses the Giant's Causeway, differs from those on other parts of the coast, by having no component prisms. It resembles a plain wall, of which the parts shiver under the hammer into very neat constituent prisms. In the dike at Seaport the same thing is observed; the prismatic structure does not penetrate two inches from its edge; the whole interior seems an amorphous mass.

The specimens of this latter dike, sent to Dr. Hope, exhibit its continuity with the adjacent basaltic rock which it traverses, and also the continuity of the fine basalt of its edge with the granular stone which composes the middle of the dike.

The dike of *Port-coon* is a very solid mass, composed of stones apparently round, and imbedded in a basaltic paste, or indurated mortar. The round stones are formed of concentric spheres, like the coats of an onion; they exceed a foot in diameter, and, together with the mortar by which they are united, they form a very compact and highly indurated rock.

Besides these large dikes, Dr. Richardson remarks, that veins from half an inch to an inch and a half thick, often cut the basaltic strata on that coast in all directions. The materials of these veins are never the same with the contiguous basalt,

from the beds of this real basalt, and pervade the supposed siliceous species; some of them connecting together the separate beds of the real basalt; others dying away in slender ramifications; as they rise through the interposed stratum. In no instance is this reversed: The veins never proceed from what is called the Siliceous Basalt. It was farther observed, that both the fracture and external surface of this stone exhibit a stratified structure, in many instances, which never happens in the true basaltes,

but

Remarks on the
basaltes of the
coast of Antrim.

but are generally finer. At Portrush is a large vein, and near it a smaller vein, not an inch thick, which, proceeding from below, terminates in the solid rock before it reaches the surface.

[
Isellanus Observations

Some of the specimens in Dr. Richardson's catalogue are from a quarry in a mass of basalt at Ballylughan, two miles south of Portrush. This basalt contains small cavities in its interior; many of them full of fresh water, which gushes out when the stone is broken by the hammer, as if it had been in a state of compression. The stone is so hard, and flies so in pieces, that Dr. Richardson has not been able to collect any of the water for the purpose of analysis.

The face of the quarry in which this variety of the basalt is found is about 15 feet high, and is cut into a stratum, the thickness of which is not yet ascertained. The rock is entirely columnar, the pillars somewhat smaller than those of the Giant's Causeway, less perfect, not articulated, sometimes bent, and variously inclined. The sides and the interior of the pillars are full of cavities. In consequence of the observations of Dr. Hamilton and Mr. Whitehurst respecting the porous texture of the air or bladder holes of the basaltes of the Causeway and its vicinity, Dr. Richardson has examined a great variety; but in no instance, except this of Ballylughan, has he found cavities, in the interior of the basaltic rocks on this coast, though they are frequent on the surface exposed to the air.

The last variety of whinstone enumerated by Dr. Richardson is the Ochrous, which makes, as he says, a conspicuous figure in the stupendous precipices along the coast of Antrim. It is disposed in extensive strata of every thickness, from an inch to twenty-four feet, and varies in colour, from a bright minium to a dull ferruginous brown.

Three remarks are made by Dr. Richardson, that are undoubtedly of importance, and show that this stone is merely basalt in a certain state of decomposition.

1. The ochrous strata are extensive; they remain always parallel to the basalt strata which they separate; they unite to the basalt without interrupting its solidity; the change from the

one to the other is sudden, and the lines of demarkation are distinct. The ochrous stone is never found but contiguous to other basalt.

2. The substances imbedded in the ochrous rock, and in basalts, are exactly the same; calcareous spar, zeolite, chalcedony, &c.

3. Among the varieties which this rock presents, there may be found every intermediate stage between sound basalt and perfect ochre. The change is often partial, beginning with veins and slender ramifications.

V.

On the Absorption of Gases by Water and other Liquids. By
JOHN DALTON.*

1. IF a quantity of pure water be boiled rapidly for a short time in a vessel with a narrow aperture, or if it be subjected to the air-pump, the air exhausted from the receiver containing the water, and then be briskly agitated for some time, very nearly the whole of any gas the water may contain, will be extricated from it.

Air or gas is extricated from water by boiling, and agitation in a vacuum.

2. If a quantity of water thus freed from air be agitated in any kind of gas, not chemically uniting with water, it will absorb its bulk of the gas, or otherwise a part of it equal to some one of the following fractions, namely, $\frac{1}{8}$, $\frac{1}{27}$, $\frac{1}{64}$, $\frac{1}{125}$, &c. these being the cubes of the reciprocals of the natural numbers 1, 2, 3, &c. or $\frac{1}{1^3}$, $\frac{1}{2^3}$, $\frac{1}{3^3}$, $\frac{1}{4^3}$, &c. the same gas always being absorbed in the same proportion, as exhibited in the following table:—It must be understood that the quantity of gas is to be measured at the pressure and temperature with which the impregnation is effected.

The volume of every gas absorbed by water is constant, and is either equal to the bulk or to the cube of a reciprocal of that bulk,

—equal pressures and temperatures being supposed.

* Manchester Mem. N. S. Vol. I.

Table of quantities.

Bulk absorbed, the bulk of water being unity. $\frac{1}{1} = 1$	Carbonic acid gas, sulphuretted hydrogen, nitrous oxide.*
$\frac{1}{2} = \frac{1}{8}$	Olefiant gas, of the Dutch chemists.
$\frac{1}{3} = \frac{1}{27}$	Oxygenous gas, nitrous gas,† carburetted hydrogen gas, from stagnant water.
$\frac{1}{4} = \frac{1}{64}$	Azotic gas, hydrogenous gas, carbonic oxide.
$\frac{1}{5} = \frac{1}{125}$	None discovered.

3. The gas thus absorbed may be recovered from the water the same in quantity and quality as it entered, by the means pointed out in the first article.

4. If a quantity of water free from air be agitated with a mixture of two or more gases (such as atmospheric air) the water will absorb portions of each gas the same as if they were presented to it separately in their proper density.

Water absorbs any gas in the same quantity, whether it contain another gas or not.

Ex. gr. Atmospheric air, consisting of 79 parts azotic gas, and 21 parts oxygenous gas, *per cent.*

Water absorbs $\frac{1}{64}$ of $\frac{79}{100}$, azotic gas = 1.234
 ————— $\frac{1}{27}$ of $\frac{21}{100}$, oxygen gas = .778

Sum, *per cent.* 2.012

* According to Mr. William Henry's experiments, water does not imbibe quite its bulk of nitrous oxide; in one or two instances with me it has come very near it: The apparent deviation of this gas, may be owing to the difficulty of ascertaining the exact degree of its impurity.

† About $\frac{1}{27}$ of nitrous gas is usually absorbed; and $\frac{1}{27}$ is recoverable: This difference is owing to the residuum of oxygen in the water, each measure of which takes $3\frac{1}{2}$ of nitrous gas to saturate it, when in water. Perhaps it may be found that nitrous gas usually contains a small portion of nitrous oxide.

2. If water impregnated with any one gas (as hydrogenous) be agitated with another gas *equally* absorbable (as azotic) there will *apparently* be no absorption of the latter gas; just as much gas being found after agitation as was introduced to the water; but upon examination the residuary gas will be found a *mixture* of the two, and the parts of each, in the water, will be exactly proportional to those out of the water. If water and gases be agitated in confinement, a mixture will take place of the gases in and out of the water, &c.

6. If water impregnated with any one gas be agitated with another gas less or more absorbable; there will *apparently* be an increase or diminution of the latter; but upon examination the residuary gas will be found a *mixture* of the two, and the proportions agreeable to article 4.

7. If a quantity of water in a phial having a ground stopper very accurately adapted, be agitated with any gas, or mixture of gases, till the due share has entered the water; then, if the stopper be secured, the phial may be exposed to any variation of *temperature*, without disturbing the equilibrium: That is, the quantity of gas in the water will remain the same whether it be exposed to heat or cold, if the stopper be air-tight. Temperature does not affect such inclosed fluids.

N. B. The phial ought not to be near full of water, and the temperature should be between 32° and 212° .

8. If water be impregnated with one gas (as oxygenous), and another gas, having an affinity for the former (as nitrous), be agitated along with it; the absorption of the latter gas will be greater, by the quantity necessary to saturate the former, than it would have been if the water had been free from gas.* Gases which are disposed to combine.

9. Most liquids free from viscosity, such as acids, alcohol, liquid sulphurets, and saline solutions in water, absorb the same quantity of gases as pure water; except they have an affinity for the gas, such as sulphurets for oxygen, &c. The absorption by other liquids is the same as by water.

The preceding articles contain the principal facts necessary to establish the theory of absorption: Those that follow are of a subordinate nature, and partly deducible as corollaries to them.

* One part of oxygenous gas requires 3.4 of nitrous gas to saturate it in water. It is agreeable to this that the rapid mixture of oxygenous and nitrous gas over a broad surface of water, occasions a greater diminution than otherwise. In fact, the *nitrous* acid is formed this way; whereas, when water is not present, the *nitric* acid is formed, which requires just half the quantity of nitrous gas, as I have lately ascertained.

Natural waters
or rain contain
the due share of
atmosf. air; but
corrupt water
has less or no
oxygen.

10. Pure distilled water, rain and spring water usually contain nearly their due share of atmospheric air: if not, they quickly acquire that share by agitation in it, and lose any other gas they may be impr/gnated with. It is remarkable, however, that water by stagnation, in certain circumstances, loses part or all of its oxygen, notwithstanding its constant exposition to the atmosphere. This I have uniformly found to be the case in my large wooden pneumatic trough, containing about eight gallons, or $1\frac{1}{2}$ cubic foot of water. Whenever this is replenished with tolerably pure rain water, it contains its share of atmospheric air; but in process of time it becomes deficient of oxygen: In three months the whole surface has been covered with a pellicle, and no oxygenous gas whatever was found in the water. It was grown offensive, but not extremely so; it had not been contaminated with any material portion of metallic or sulphureous mixtures, or any other article to which the effect could be ascribed.* The quantity of azotic gas is not materially diminished by stagnation, if at all.—These circumstances, not being duly noticed, have been the source of great diversity in the results of different philosophers upon the quantity and quality of atmospheric air in water. By article 4, it appears that atmospheric air expelled from water ought to have 38 per cent. oxygen; whereas by this article air may be expelled from water that shall contain from 38 to 0 per cent. of oxygen. The disappearance of oxygenous gas in water, I presume, must be owing to some impurities in the water which combine with the oxygen. Pure rain water that had stood more than a year in an earthenware bottle had lost none of its oxygen.

Why water by
agitation absorbs
most oxygen
from air.

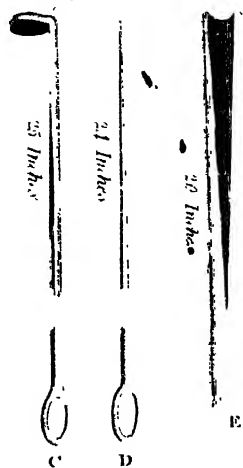
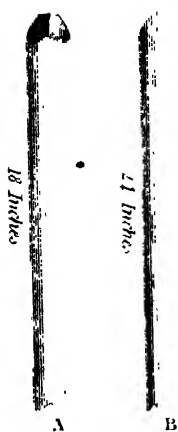
11. If water free from air be agitated with a small portion of atmospheric air (as $\frac{1}{15}$ of its bulk) the residuum of such air will have proportionally less oxygen than the original: If we take $\frac{1}{15}$, as above, then the residuum will have only 17 per cent. oxygen; agreeably to the principle established in article 4. This circumstance accounts for the observations made by Dr. Priestley, and Mr. William Henry, that water absorbs oxygen in preference to azote.

Disappearance of
gas by agitation
under a jar.

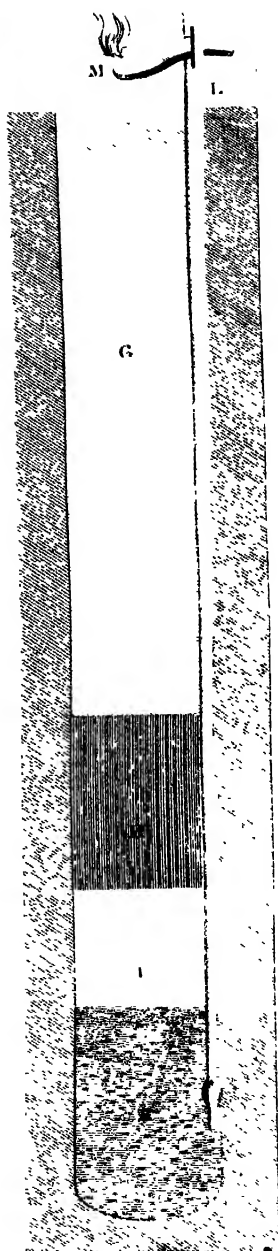
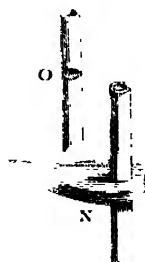
12. If a tall glass vessel, containing a small portion of gas be inverted into a deep trough of water, and the gas thus confined by the glass and the water be briskly agitated, it will gradually disappear.

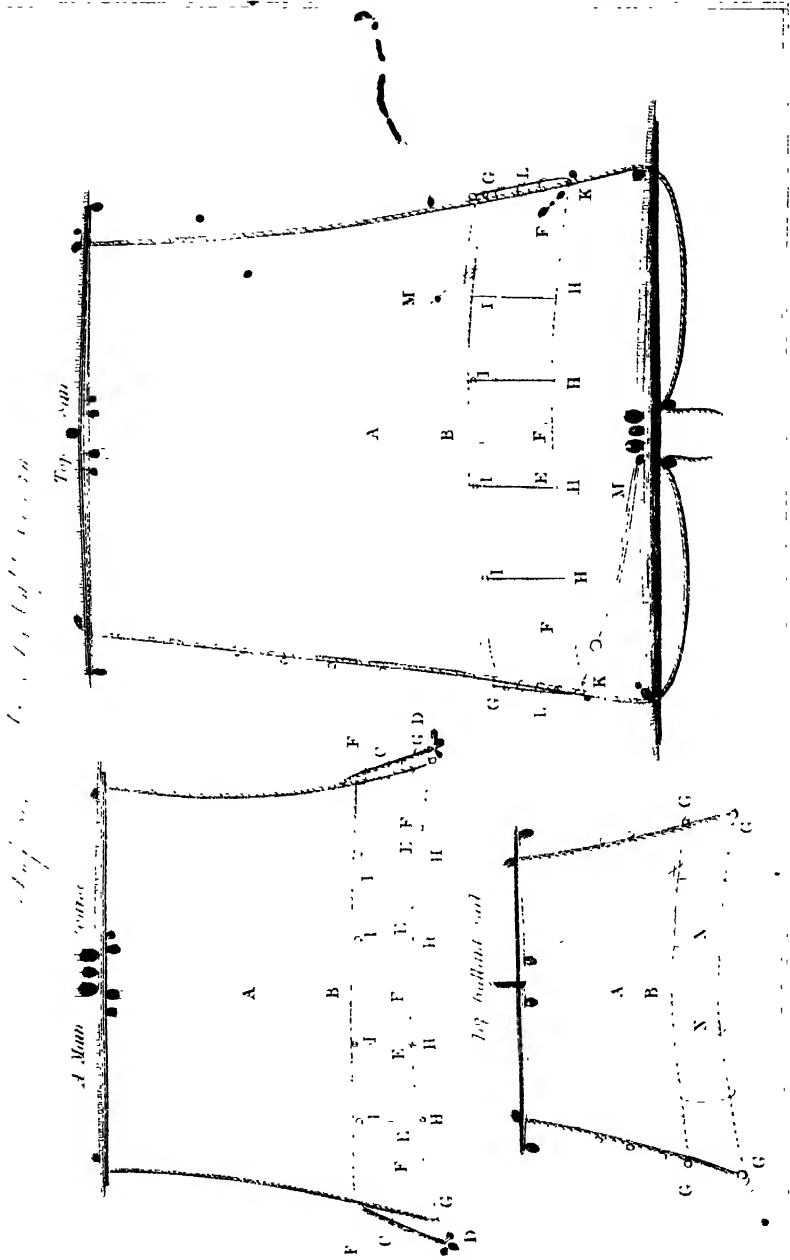
* It was drawn from a leaden cistern.

Method of blasting rocks.



Small holes by the mouth





It is a wonder that Dr. Priestley, who seems to have been the first to notice this fact, should have made any difficulty of it;—the loss of gas has evidently a mechanical cause; the agitation divides the air into an infinite number of minute bubbles, which may be seen pervading the whole water; these are successively driven out from under the margin of the glass into the trough, and to escape.

13. If old stagnant water be in the trough, in the last experiment, and atmospheric air be the subject, the oxygenous gas will very soon be almost wholly extracted, and leave a residuum of azotic gas; but if the water be fully impregnated with atmospheric air at the beginning, the residuary gas examined at any time will be pure atmospheric air.

14. If any gas not containing either azotic or oxygenous gas, be agitated over water containing atmospheric air, the residuum will be found to contain both azotic and oxygenous gas.

Old stagnant water.

Agitation of gas over common water gives out oxygen and azote by mixture.

15. Let a quantity of water contain equal portions of any two or more unequally absorbable gases: For instance, azotic gas, oxygenous gas, and carbonic acid gas; then, let the water be boiled or subjected to the air-pump, and it will be found that unequal portions of the gases will be expelled. The azotic will be the greatest part, the oxygenous next, and the carbonic acid will be the least. For, the previous impregnation being such as is due to atmospheres of the following relative forces nearly:

The escape of any gas from water by removing the pressure will be greater the less absorbable.

Azotic - - - 21 inch. of mercury.

Oxygenous - - 9

Carbonic acid - 1

consequently, when those forces are removed, the resiliency of the azotic gas will be the greatest, and that of the carbonic acid the least; the last will even be so small as not to overcome the cohesion of the water without violent agitation.

Remarks on the Authority of the preceding Facts.

In order to give the chain of facts as distinct as possible, I have not hitherto mentioned by whom or in what manner they were ascertained.

The fact mentioned in the first article has been long known; a doubt, however, remained respecting the quantity of air still

Remarks on the laws of absorption of gases by dense fluids, &c.

Remarks on
the laws of ab-
sorption of gases
by dense fluids,
&c.

left in water after ebullition and the operation of the air-pump. The subsequent articles will, I apprehend, have placed this in a clearer point of view.

In determining the quantity of gases absorbed, I had the result of Mr. William Henry's experience on the subject before me, an account of which has been published in the Philosophical Transactions for 1803. By the reciprocal communications since, we have been enabled to bring the results of our experiments to a near agreement; as the quantities he has given in his appendix to that paper nearly accord with those I have stated in the second article. In my experiments with the less absorbable gases, or those of the 2d, 3d, and 4th classes, I used a phial holding 2700 grains of water, having a very accurately ground stopper; in those with the more absorbable of the first class, I used an eudiometer tube, properly graduated, and of aperture so as to be covered with the end of a finger. This was filled with the gas and a small portion expelled by introducing a solid body under water; the quantity being noticed by the quantity of water that entered on withdrawing the solid body, the finger was applied to the end and the water within agitated; then removing the finger for a moment under water, an additional quantity of water entered, and the agitation was repeated till no more water would enter, when the quantity and quality of the residuary gas was examined. In fact, water could never be made to take its bulk of any gas by this procedure; but if it took $\frac{1}{16}$, or any other part, and the residuary gas was $\frac{1}{3}$ pure, then it was inferred that water would take its bulk of that gas. The principle was the same in using the phial; only a small quantity of the gas was admitted, and the agitation was longer.

There are two very important facts contained in the second article. The first is, that the quantity of gas absorbed is as the density or pressure. This was discovered by Mr. William Henry, before either he or I had formed any theory on the subject.

The other is, that the density of the gas in the water has a special relation to that out of the water, the distance of the particles within being always some multiple of that without: Thus, in the case of carbonic acid, &c. the distance within and without is the same, or the gas within the water is of the same density as without; in olefiant gas the distance of the

particles

particles in the water is twice that without; in oxygenous gas, &c. the distance is just three times as great within as without; and in azotic, &c. it is four times. This fact was the result of my own enquiry. The former of these, I think, decides the effect to be mechanical; and the latter seems to point to the principle on which the equilibrium is adjusted.

Remarks on
the laws of ab-
sorption of gases
by dense fluids,
&c.

The facts noticed in the 4th, 5th and 6th articles, were investigated *a priori* from the mechanical hypothesis, and the notion of the distinct agency of elastic fluids when mixed together. The results were found entirely to agree with both, or as nearly as could be expected from experiments of such nature.

The facts mentioned in the 7th article are of great importance in a theoretic view; for, if the quantity of gas absorbed depend upon mechanical principles; it cannot be affected by temperature in confined air, as the mechanical effect of the external and internal air are alike increased by heat, and the density not at all affected in these circumstances. I have tried the experiments in a considerable variety of temperature without perceiving any deviation from the principle. It deserves further attention.

If water be, as pointed out by this essay, a mere receptacle of gases, it cannot affect their affinities: hence what is observed in the 8th article is too obvious to need explanation.— And if we find the absorption of gases to arise not from a chemical but a mechanical cause, it may be expected that all liquids having an equal fluidity with water, will absorb like portions of gas. In several liquids I have tried, no perceptible difference has been found; but this deserves further investigation.

After what has been observed, it seems unnecessary to add any explanation of the 10th and following articles.

Theory of the Absorption of Gases by Water, &c.

From the facts developed in the preceding articles, the following theory of the absorption of gases by water seems deducible.

1. All gases that enter into water and other liquids by means of pressure, and are wholly disengaged again by the removal of that pressure, are *mechanically* mixed with the liquid, and not *chemically* combined with it.

2. Gases

Remarks on
the laws of ab-
sorption of gases
by dense fluids,
&c.

2. Gases so mixed with water, &c. retain their elasticity or repulsive power amongst their own particles, just the same in the water as out of it, the intervening water having no other influence in this respect than a mere vacuum.

3. Each gas is retained in water by the pressure of gas of its own kind incumbent on its surface abstractedly considered, no other gas with which it may be mixed having any permanent influence in this respect.

4. When water has absorbed its bulk of carbonic acid gas, &c. the gas does not press on the water at all, but presses on the containing vessel just as if no water were in. When water has absorbed its proper quantity of oxygenous gas, &c. that is, $\frac{1}{27}$ of its bulk, the exterior gas presses on the surface of the water with $\frac{26}{27}$ of its force, and on the internal gas with $\frac{1}{27}$ of its force, which force presses upon the containing vessel, and not on the water. With azotic and hydrogenous gas the proportions are $\frac{5}{24}$ and $\frac{1}{24}$ respectively. When water contains no gas, its surface must support the whole pressure of any gas admitted to it, till the gas has, in part, forced its way into the water.

5. A particle of gas pressing on the surface of water is analogous to a single shot pressing upon the summit of a square pile of them. As the shot distributes its pressure equally amongst all the individuals forming the lowest stratum of the pile, so the particle of gas distributes its pressure equally amongst every successive horizontal stratum of particles of water downwards till it reaches the sphere of influence of another particle of gas. For instance; let any gas press with a given force on the surface of water, and let the distance of the particles of gas from each other be to those of water as 10 to 1; then each particle of gas must divide its force equally amongst 100 particles of water, as follows:—It exerts its immediate force upon 4 particles of water; those 4 press upon 9, the 9 upon 16, and so on according to the order of square numbers, till 100 particles of water have the force distributed amongst them; and in the same stratum each square of 100, having its incumbent particle of gas, the water below this stratum is uniformly pressed by the gas, and consequently has not its equilibrium disturbed by that pressure.

6. When water has absorbed $\frac{1}{27}$ of its bulk of any gas, the stratum of gas on the surface of the water presses with $\frac{26}{27}$ of

its force on the water, in the manner pointed out in the last article, and with $\frac{1}{17}$ of its force on the uppermost stratum of gas in the water: The distance of the two strata of gas must be nearly 27 times the distance of the particles in the incumbent atmosphere, and 9 times the distance of the particles in the water. This comparatively great distance of the inner and outer atmosphere arises from the great repulsive power of the latter, on account of its superior density, or its presenting 9 particles of surface to the other 1. When $\frac{1}{84}$ is absorbed, the distance of the atmospheres becomes 64 times the distance of two particles in the outer, or 16 times that of the inner. The annexed views of perpendicular and horizontal strata of gas in and out of water, will sufficiently illustrate these positions.

Remarks on the laws of absorption of gases by dense fluids, &c.

7. An equilibrium between the outer and inner atmospheres can be established in no other circumstance than that of the distance of the particles of one atmosphere being the same or some multiple of that of the other; and it is probable the multiple cannot be more than 4. For in this case the distance of the inner and outer atmospheres is such as to make the perpendicular force of each particle of the former on those particles of the latter that are immediately subject to its influence, physically speaking, equal; and the same may be observed of the small lateral force.

8. The greatest difficulty attending the mechanical hypothesis, arises from different gases observing different laws.—Why does water not admit its bulk of every kind of gas alike? This question I have duly considered, and though I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases: those whose particles are lightest and single being least absorbable, and the others more, according as they increase in weight and complexity.* An enquiry into the relative weights of the ultimate particles of bodies, is a subject, as far as I know, entirely new: I have lately been prosecuting this enquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments.

* Subsequent experience renders this conjecture less probable.

Weights of the particles of bodies. *Table of the relative weights of the ultimate particles of gaseous and other bodies.*

Hydrogen	-	-	-	-	-	-	1
Azote	-	-	-	-	-	-	4.2
Carbon	-	-	-	-	-	-	4.3
Ammonia	-	-	-	-	-	-	5.2
Oxygen	-	-	-	-	-	-	5.5
Water	-	-	-	-	-	-	6.5
Phosphorus	-	-	-	-	-	-	7.2
Phosphuretted hydrogen	-	-	-	-	-	-	8.2
Nitrous gas	-	-	-	-	-	-	9.3
Ether	-	-	-	-	-	-	9.6
Gaseous oxide of carbon	-	-	-	-	-	-	9.8
Nitrous oxide	-	-	-	-	-	-	13.7
Sulphur	-	-	-	-	-	-	14.4
Nitric acid	-	-	-	-	-	-	15.2
Sulphuretted hydrogen	-	-	-	-	-	-	15.4
Carbonic acid	-	-	-	-	-	-	15.3
Alcohol	-	-	-	-	-	-	15.1
Sulphureous acid	-	-	-	-	-	-	19.9
Sulphuric acid	-	-	-	-	-	-	25.4
Carburetted hydrogen from stagnated water	-	-	-	-	-	-	6.3
Olefiant gas	-	-	-	-	-	-	5.3

VI.

On the supposed fascinating Power of the Rattle-snake. With a remarkable Indian Tradition upon which it is probable the early European Settlers founded their popular Tales. From the Philadelphia Medical and Physical Journal, by BENJAMIN SMITH BARTON, M. D.

Fascinating power of the rattle-snake described by Fabricius.

ALMOST all amphibious animals (says Professor Fabricius,) the tortoise excepted, live by preying upon other animals. But being destitute of strength and swiftness, nature has given, at least to some of them (according to the testimony of many and creditable writers,) the peculiar faculty of forcing other animals to throw themselves into their open jaws. Kalm, the Swede, and the American Smith Barton, attest of the American

American serpents, that if they fix their fiery, glaring eyes upon any animal, such as a squirrel, or a bird, within a certain distance, they entirely lose the power of escaping, but throw themselves, slowly, irresistibly, into the extended jaws of the snake. And if any thing disturbs the snake, so that it withdraws its eyes but for one moment, they escape with the utmost precipitation.

We observe (continues this learned naturalist) something similar to this in our common, tardy, thick, and fat toads, which frequently sit under little stones and bushes, having their mouths wide open, into which flies, bees, and other insects, are drawn in the same manner. All the theories that have hitherto been offered to explain these appearances appear to me both unnatural and improbable. Indeed, I cannot but doubt the reality of the fact itself, until we shall receive further observations and discoveries relative to it.

J. C. FABRICII, &c.

Reſultate Natur-Hiſtoriſcher Vorleſungen,
p. 267, 268. Kiel: 1804.

It will be evident to any one, who has perused, with at-
tention, my two publications * on the supposed fascinating
faculty of the rattle-snake, and other American serpents, that
Mr. Fabricius has by no means fully comprehended my pecu-
liar theory. I have not adopted the hypothesis of the very
respectable Kalm, with whose name mine is mentioned by
the Danish Professor. On the contrary, I have endeavoured
to show, and I flatter myself that I have very satisfactorily
shown, that there is no solid foundation for the vulgar, and
very generally-received opinion, that serpents are endued
with the faculty of fascinating, or charming, other animals.

Annotation by
Dr. Barton.

B. S. B.

The following very curious tradition of some of our Indians, Narrative.
relative to serpents, is worthy of publication in this place.
A part of the tradition has already been published in my

* A Memoir concerning the Fascinating Faculty which has been
ascribed to the Rattle-snake, and other American Serpents. Phi-
ladelphia: 1796.—Supplement to a Memoir, &c. Philadelphia,
1800.—Or see Philof. Journal, Vols. VII. and VIII.

Supplement

Supplement to a Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-snake, and other American Serpents.

The rattle-snake catches its prey by craft and address.

Having questioned Indians, a number of times, with respect to snakes having the power of charming, and always being answered in the negative, I was at length desired (says my friend, Mr. John Heckewelder) to give the reason the white people had for believing such a thing, which not being satisfactory, Pemaholend * declared: "The rattle-snake obtains its food merely by slyness, and a persevering patience. It knoweth as well where to watch for its prey as a cat does, and succeeds as well. It has, and retains its hunting grounds. In spring, when the warm weather sets in, and the woods seem alive with the smaller animals, it leaves its den. It will cross a river, and go a mile and further from its den, to the place it intends to spend the summer; and in fall, when all the young animals bred this season are become strong and active, so that they are no more so easily overtaken or caught, it directs its course back again, to its den, the same as a hunter does to his camp.

Indian tradition. "The white-people," continued Pemaholend, "probably have taken the idea of this snake having the power of charming from a tradition of ours (the Indians) which our forefathers have handed down to us, from many hundred years back, and long before ever the white people came into this country. Then (they tell us) there *was* such a snake, and a rattle-snake too, but then there was only *this one* snake which had this power, and he was afterwards destroyed; and since that time it hath never been said that any other of the kind had made its appearance."

American native tradition about a rattle-snake.

"At my request, Pemaholend related the tradition, and in the following words. "Our forefathers have told us, that at a small lake, or large pond, not a great distance from where, as is believed, now the great city *Quequenaku* (Philadelphia) is built, there dwelt a rattle-snake, whose length and thickness exceeded that of the thickest and longest tree in the woods. This snake was very destructive, not only in destroying so much game, but in devouring so many Indians: for when he was hungry, he only looked round, and whatever he

* An aged and much respected Delaware-Indian.

law, whether Indian, deer, turkey, or even geese flying, he only held his head that way, opening his mouth wide, and drawing breath in the manner we do, and nothing could prevent such living creature entering his jaws. It is even said, that a whole flock of geese, flying at a great distance, have been drawn into his mouth, at one time; * and it was well known among the Indians, that of all the hunters or travellers, who passed that way, very few escaped him.

American native
tradition about
a rattle-snake.

"The Indians well knew when he was hungry, for then he grew angry, and blew with his mouth, which sounded like thunder: for his breath was so powerful, that all the trees, however large, would bend, and even sometimes break down before him. There being no prospect of ever killing him with arrows, on account of the barrenness of the land far round the lake, into which he would always retire, after satisfying his hunger, a great council of the nation was called together, and the question put, *Where are the Munittoes of the nation? Are they no more? Shall the whole of the nation be destroyed by a Mannitto-Snake?* At length, two young men, endowed with *Munittioie powers*, offered their services, and declared, that unless the Mannittoie power of the snake exceeded theirs, they should succeed; but they would, at all events, make an attempt. They then bid farewell to the assembly and their friends, dived into the river, from whence they proceeded under the water to a place opposite the Mennúppæc (lake, or large pond) where this snake dwelt. They made an opening under ground, from the river to the centre of the pond, by which the pond was drained, and became perfectly dry. After returning again, the same way they had come, they found the snake in great uneasiness, and on dry ground. Taking then the advantage of the dry weather, and the grass far around the snake being dry, they set fire to the grass, at a distance, and around the snake, by which means he was burnt

* It is curious, at least, to compare this part of the Indian tradition with what Metrodorus, as cited by Pliny, relates of certain Asiatic serpents. These, he says, by means of their breath, attracted birds, however high they were, or however quick their flight. "Metrodorus, circa Rhyndacum amnem in Ponto, ut supervolantes quamvis alte perniciterque, alites haustu raptas absorbeant." Plin. Hist. Nat. lib. viii. cap. 14.

American native to death.* Thus (continued Pemaholend) was the monster. tradition about a killed by two mannitto men of the nation: for, you must rattle-snake. know, in those days, we had such men among us, who could live as well in the water as on land."

"Conversing one day with a Monfy (advanced in years) on ancient times, on the migration of the Indians, &c. he, in order to convince me (says Mr. Heckewelder) what the Indians once were, mentioned the killing of the big snake, the history of which, according to his relation, differing only in the following points:

"a. He did not think it had been a rattle-snake, but understood the old men, from whom he had heard it so often related (when he was young), that it was a snake of a peculiar kind, and had feet; and that never since had a snake of this kind appeared:

"b. That he was not sure as to the place where this snake kept; believed it had been higher up the country, and kept in a wide and deep place of the river, and in the country of the Munsees (or Minfy) and was killed by a Mannitto Munsee:

"c. That after the nation had met in council, and the above questions put, a Munsee man of no character, nor seemingly of any consequence to the nation, said and declared, that he had *Mannittoie Powers*; could and would destroy the monster, prescribing the ceremonies the assembly were to observe during the expedition. That he then made a very strong arrow, or spear, sharp at both ends; and being equipped, took leave of the assembly—plunged into the river, and dived under water, until he arrived within a small distance of the place where the snake lay, or floated, basking in the sun. Here he ascended to the surface, and calling out to the snake to receive him, he opened his mouth wide, and drew him in, when, however, in an instant, the snake was stabbed by him through both his sides, with the spear, which wounded him so deadly, that he gave a whirl, and being under great pain, discharged his excrements, and with the same this hero, who

* Even this part of the Indian tradition seems to be borrowed from the old world. See a curious relation of the capture of an enormous serpent in *The Life of Sethos, as taken from private memoirs of the Egyptians*. Vol. i. p. 125—147. London: 1737.

then swam again to shore, announcing his victory, and congratulating the assembly on the deliverance of the nation.

"Thus (continued the old Munfee) were the Indians of those days *Munnittoes*. Nothing could resist them. They knew nothing of drowning. Our first Parents have sprung from the bottom of a lake!"

VII.

*A Description of a Property of Caoutchouc, or Indian Rubber; With some Reflections on the Cause of the Elasticity of this Substance. In a Letter to Dr. HOLME.**

SIR, *Middleshaw, near Kendal, Nov. 26, 1802.*

THE substance called Caoutchouc, or Indian Rubber, possesses a singular property; which, I believe, has never been taken notice of in print, at least by any English writer; the present letter contains my experiments and reflections on the subject; and should they appear to deserve the attention of your philosophical friends, I am certain you will take the trouble of communicating the paper to the Literary and Philosophical Society of Manchester.

The property I am about to describe depends on the temperature of the Caoutchouc, which is used in the experiment; for heat increases the pliancy of the substance, and cold, on the contrary, renders it more rigid: so that when a slip of this resin has been sufficiently warmed, it may be extended to more than twice its natural length, by a moderate force applied to its extremities, after which it will recover its original dimensions in a moment, provided one of the ends of it be let go as soon as it has been stretched. This disposition of the substance may be produced by a degree of temperature less than the heat of the blood; it is therefore necessary to prepare a slip of it, by steeping it for a few minutes in warm water, or by holding it somewhat longer in the fist; either of these precautions makes the resin pliant, and fits it for the experiment; which is performed in the following manner.

Caoutchouc
more pliant by
heat,

* Manchester Mem. N. S. Vol. I.

and less dense.

I made a piece of Caoutchouc a little heavier than an equal bulk of water, the temperature of which was 45 degrees; the vessel containing the resin and water was then placed on the fire; and when the contents of it were heated to 130 degrees, the Caoutchouc floated on the surface.

It becomes cold by sudden drawing out, and hot by contraction.

Exp. 1. Hold one end of the slip, thus prepared, between the thumb and fore-finger of each hand; bring the middle of the piece into slight contact with the edges of the lips; * taking care to keep it straight at the time, but not to stretch it much beyond its natural length; after taking these preparatory steps, extend the slip suddenly; and you will immediately perceive a sensation of warmth in that part of the mouth which touches it, arising from an augmentation of temperature in the Caoutchouc: for this resin evidently grows warmer the further it is extended; and the edges of the lips possess a high degree of sensibility, which enables them to discover these changes with greater facility than other parts of the body. The increase of temperature, which is perceived upon extending a piece of Caoutchouc, may be destroyed in an instant, by permitting the slip to contract again; which it will do quickly by virtue of its own spring, as oft as the stretching forth ceases to act as soon as it has been fully exerted. Perhaps it will be said, that the preceding experiment is conducted in a negligent manner; that a person, who wishes for accuracy, will not trust his own sense of feeling in inquiries of this description, but will contrive to employ a thermometer in the business. Should the objection be started, the answer to it is obvious; for the experiment in its present state demonstrates the reality of a singular fact; by convincing that sense, which is the only direct judge in the case, that the temperature of a piece of Caoutchouc may be changed, by compelling it to change its dimensions. The use of a thermometer determines the relative magnitudes of these variations, by referring the question of temperature to the eye; experiments of this sort are therefore of a mathematical nature, and afford a kind of knowledge with which we have nothing to do at present; for we are not inquiring after proportions, but endeavouring to esta-

* This effect was first noticed in 1784, at Mr. Kirwan's meetings in Newman street, and Dr. Crawford ascribed it to change of capacity similar to what he supposed to take place in a nail by hammering.—N.

to blith the certainty of a fact, which may assist in discovering the reason of the uncommon elasticity observable in Caoutchouc. My essay or letter appears to be running into a long digression; the subject must therefore be resumed, and it will not be improper to premise the following simple experiment, in the present state of the inquiry; because it seems capable of affording no inconsiderable degree of insight into the plan which nature pursues in producing the phenomenon in question.

Exp. 2. If one end of a slip of Caoutchouc be fastened to a rod of metal or wood, and a weight be fixed to the other extremity, in order to keep it in a vertical position; the thong will be found to become shorter with heat and longer with cold. The processes of heating, cooling, and measuring bodies are so well known, that I need not enter into the minuter parts of the experiment; it will be proper, however, to add, that an increase of temperature diminishes the specific gravity of the Indian Rubber, and a loss of heat occasions a contrary effect in it; as I have proved experimentally. The knowledge of the latter fact leads me to conclude, apparently on reasonable grounds, that the pores or interstices of Caoutchouc are enlarged by heat, and diminished by cold; consequently when a slip of this substance which remains extended by a weight, or the application of force, happens to contract from an accession of temperature, the capacity of its pores, taken separately or collectively, is augmented by the change that takes place in the figure of the thong. Now if the existence of caloric be admitted, it will follow from the preceding arguments, that the phenomenon under consideration is occasioned by the alternate absorption and emission of the caloric fluid, in the same manner that ropes, the blades of Fuci, as well as many more bodies, are obliged to contract and extend themselves, by the alternate absorption and emission of water.—You will perceive by the tenour of the foregoing observations, that my theory of this case of elasticity is perfectly mechanical; in fact, the explanation of it depends upon the mutual attraction of Caloric and Caoutchouc; the former of which penetrates the latter, and pervades every part of it with the greatest ease and expedition; by which the resin is compelled to accommodate its pores to that portion of the Caloric fluid which is due to its whole mass.

Caoutchouc when stretched expands by heat and contracts by cold.

Theory. This is affected by caloric, as ropes are by water.

at any particular degree of temperature. In order to apply the last remark to the phenomenon under consideration I may observe, that if a force be exerted on a piece of Caoutchouc to alter the dimensions of its pores, the mutual attraction mentioned above will resist the effort. But the ease with which this substance may be made to change its figure, and the retractile power which it possesses on these occasions, shew that its constituent particles move freely amongst themselves: but where there is motion, there is void space; consequently Caoutchouc abounds with innumerable pores or interstices, the magnitudes of which are variable, because the specific gravity of the resin becomes less with heat, and greater

that its capacity may be mechanically altered and the caloric extruded, &c.

with cold. Now if the dimensions of the pores in a piece of Caoutchouc can be lessened, without taking away part of the matter, of heat, which it contains at the time; this new arrangement in the internal structure of the slip will lessen its capacity for the matter of heat, and consequently augment its temperature. But the warmth of such a slip is increased by stretching it, according to the first experiment; the pores of it are therefore diminished: and the effort, which it exerts at the time, arises from the mutual attraction of the Caoutchouc and Caloric; which attraction causes an endeavour to enlarge the interstices of the former for the reception of the latter; hence it happens that the thong contracts longitudinally, according to the second experiment, and the redundant caloric is absorbed in the course of this operation, which again reduces the temperature. The preceding explanation agrees very well with the phenomenon, as it is stated in the beginning of this letter; and the theory receives additional confirmation from the following facts.

Overstretched Caoutchouc does not completely recover itself in the cold; but heat restores its elasticity:

Exp. 3. If a thong of Caoutchouc be stretched in water warmer than itself, it retains its elasticity unimpaired; on the contrary, if the experiment be made in water colder than itself, it loses part of its retractile power, being unable to recover its former figure; but let the thong be placed in hot water, while it remains extended for want of spring, and the heat will immediately make it contract briskly. The foregoing circumstances may be considered as proving, that the elasticity of Caoutchouc is not a constitutional quality of the substance, but a contingent effect, arising from the loss of equilibrium between the portion of caloric, which the resin

whence the nature of its elasticity is deduced, &c.

resin happens to contain at any moment, and its capacity to receive that fluid at the same instant. The object of the present letter is to demonstrate, that the faculty of this body to absorb the calorific principle, may be lessened, by forcibly diminishing the magnitudes of its pores; and this essential point of the theory may be confirmed by experiment: for the specific gravity of a slip of Caoutchouc is increased, by keeping it extended, while it is weighed in water.

JOHN GOUGH.

VIII.

*Observations on the training of Pugilists, Wrestlers, Jockies, and others, who give themselves up to Athletic Exercises; with some Queries for discovering the Principles thereof, and the Process of training Running Horses, &c. with a View of ascertaining whether the same can furnish any Hints serviceable to the Human Species.**

PROFESSIONAL men are ready to acknowledge, that prevention is better than cure; and the best informed ingenuously admit, that organic diseases, once confirmed, are beyond the reach of their art. As organic diseases generally proceed from slow and gradual changes, they may certainly be prevented by temperance and labour; by activity of body, and contentment of mind. In regard to the common metaphysical expressions, "of the exhausting of the excitability; of the wearing of the parts; of the attrition of our fluids, in circulation, against the solids; of the abrasion of the solids by fric-

General considerations on organic diseases.

* The subsequent queries and observations have been circulated by Sir John Sinclair, with a view to obtain information concerning the effects of diet and exercise on the human frame, from a class of practical experimentalists, whom the pride of science has hitherto overlooked. The philosophical manner in which this branch of diastatic medicine is here considered, appears to render it a fit subject for insertion in a Journal conducted on the plan of the present. In promoting the circulation of this paper, we have no doubt that we are coinciding with the plan of the author, by extending his means of information: Any communications tending to throw further light on the subject, will be acceptable. W. N.

tion; of the debility produced by the most natural powers supporting life, namely, the waste of substance created by that exercise and labour, for which we seem peculiarly destined,"—all these expressions are extremely suspicious. The speculator is always to be suspected, when, forsaking plain direct facts, he involves his want of meanings and conscious ignorance, in learned words, or metaphor.

It is usually supposed that bones, and other solid parts are permanent; but they are successively replaced, like the fluids.

These metaphorical expressions have originated in a persuasion, that the bones, cartilages, muscles, and other solid parts, being once formed, are *permanent*, because the identity of the individual is permanent; and that being *once* formed, and always retaining one shape, their actual component parts must continue the same. Nothing in philosophy is farther from the truth. There are experiments to demonstrate, that every part and particle of the firmest bones, is successively absorbed and deposited again*. The solids of the body, whatever their form or texture, are incessantly renewed. The whole body is a perpetual secretion, and the bones and their ligaments, the muscles and their tendons, all the finer and all the more flexible parts of the body, are as continually renewed, and as properly a secretion, as the saliva that flows from the mouth, or the moisture that bedews the surface. The health of all the parts, and their soundness of structure, depends on this perpetual absorption, and perpetual renovation; and exercise, by promoting at once absorption and secretion, promotes life, without hurrying it; renovates all the parts and organs, and preserves them apt and fit for every office.

Nutrition is a general process.

Nutrition belongs not to the stomach alone, which but prepares the food, and converts it into chyle, but to the vessels by which it is circulated, and appropriated to the nutrition of parts, which of course is performed by every petty artery of the body.

Many general rules are rash and dangerous.

In nothing should we be more anxiously careful, than, in laying down rules, which must affect the health of thousands; and whenever we proceed on doctrines, unsupported by fact,

* This has been ascertained by giving madder to growing animals, especially pigs and fowls, among their food. It is found that the madder tinges the bones, layer after layer, with a red colour; and by the deepness of the tinge, demonstrates the succession in which the particles of the bone are absorbed and deposited. This is, I believe, the conclusion which physiologists have formed.

wherever

wherever we divert mankind from those amusements and labours to which nature excites us, we should proceed with particular caution. We read in books, that life and the body are but as a given quantity of living energy and living materials, to be expended and used with discretion and economy; and that the sum of excitability, which is born with the child, is expended towards the close of life. The doctrine of abrasion also intimates, that our solids are perpetually wasting, and that it is by the diminution of moisture,—the aridity of solids, the scantiness of fluids, and the slow induration of the solid parts; that the body becomes shrunk, emaciated, stiff, and motionless, before it sinks into the grave. And, rash as the doctrine seems, it has been boldly asserted, that “to live with as little food, and as little exercise as possible, is the surest means to preserve the body, and to live long.” To live with as little food, and as little exercise as possible, would make a man little better than a mere grasshopper. A man living thus, would be a voluntary prisoner, wan, colourless, fleshless, bloodless, having no speculation in his eyes, no marrow in his bones; his complexion would declare him what he was. This system practised, either in infancy, in the prime of manhood, or in the decline of life, would abridge it. Ascetics are a proof, not of the length of life, which temperance insures, but of the premature old age which abstinence brings upon us. The squalid look, the hollow cheek, the matted hair, the emaciated body, only prove how much, by such criminal self-denial, the body suffers, with but little profit to the powers of the mind. Let us then take care that our philosophy be not too severe; for men may run into real danger, if we take from them every fair indulgence, or divert them from following the dictates of nature. The fairest livers, who have not abused, but have enjoyed their strength and health, have in general enjoyed them longest.

The doctrine of abrasion or wearing out has been absurdly applied.

There are habits which seem to be natural to, and congenial with, the several periods of life. The child should merely suck, sleep, and vegetate. The boy should ramble wild and unconstrained, little oppressed with tasks or studies, and nourished with abundance of simple food. The youth should be emperate, sober, active. The old man quiet, sedate, self-indulgent; should have long sleep, delicate food, rich wines, and agreeable temperature; little labour, and a cheerful mind.

Natural habits of infancy, youth, &c. &c.

Nature assigns us vigour, spirit, enterprise, and foresight in the early part of life, to treasure up the needful indulgences for age. Parents are careful of our first infancy; we ourselves ought to provide for our latter childhood.

Considerations
respecting the
functions of the
skin;—

The most intelligent professional men have an opinion concerning the functions of the skin, consonant with that of the vulgar; and more refined, only from their assigning a general cause for those effects, of which all of us are conscious. The skin is not regarded merely as an organ of secretion, destined for draining off superfluous moisture, or saline particles, from the general mass of fluids, but as a surface of more active circulation, which solicits the blood to the very extremities of the vessels, and thus contributes to support and complete the circulation of the blood, and to nourish the parts within. The skin is regarded as connected, in a peculiar manner, with all the parts of the cellular substance, interposed betwixt the muscles, and involving the blood vessels. The state of the skin indicates the condition of that cellular substance, whose office it is to conduct the blood-vessels to all parts, especially to the muscular flesh, and to nourish the parts; and while the circulation of the skin is lively and active, that of the involved parts can never flag. The condition of the bowels, and of the skin, are the first and most natural points for the physician to attend to. It is by regulating these, that he regulates the pulse; by stimulating or soothing them, that he raises or depresses the vital actions; and it is matter of common observation, that in animals, a good skin is the criterion of health, and the dryness of the skin, the forming of scabs or eruptions upon it, and the clapping of the hair, (as it is called by those who have the care of stock), are the first and surest signs of approaching disease.

—and the intestines.

The lungs and
their office.

Next to the free circulation of the blood through all the body, terminating in the surface, that of the free transit of the blood through the lungs, is essential to health.

The oxydation or chemical change produced by air upon the blood, is essential to its vital properties. A free and powerful respiration is most essential to a fresh colour of the face to lively spirits, and cheerful feelings, and to the healthy and vigorous actions of the body. "It is my breathing hour of the day," says Hamlet to Ofrio. It is a princely thing to set apart hours for exercises; and there is little doubt, that if all those

those, who linger away their hours in luxurious and indolent relaxations, were to assign a regular portion of their time to the hardy and manly exercises of walking, riding, fencing, &c. and would take *their breathing hour*, they would breathe long and well.

These reflections naturally arise upon considering the almost incredible perfection, to which those, whose profession it is to train men to athletic exercises, have brought their respective arts. By certain processes, they improve the breath, the strength, and the courage of those they take in hand, so as to enable them to run thirty, or walk a hundred miles, in a given space of time; to excel in wrestling; or to challenge a professed boxer. Would it not then be a most important addition to the facts we already know concerning the means of improving strength, and ensuring long life, if authentic information could be procured from those districts where athletic exercises prevail, what are esteemed the best and surest processes for training men for foot-races, trials of strength in wrestling or boxing matches, or for raising the strength and courage of game-cocks, or improving the wind, strength, and speed of running horses to their highest pitch.*

Those who give themselves out as skilful in this art, attend to the state of the bowels, the skin, and the lungs. They use such means as reduce the cellular or fatty substance, and invigorate the muscular fibres. When they take a man in training for any feat of this kind, he is not oiled and suppled as the ancient athletes were; for as their common modes of life were hardy and active, they needed no other preparation: but he is sweated, purged, and dieted, and then put upon trial. He is purged with very drastic purges, to reduce his grossness. He is made to walk out under a load of clothes; his walks are regularly increased, and a certain number of times a-week; he is laid between two feather-beds; sweat promoted by drinks; his limbs taken from between the feather-beds, successively, and rubbed very roughly. After enduring for many

* Though not immediately connected with the object of this paper, it may not be improper to suggest, that it would be of great importance, if medical gentlemen, whether in the army or navy, who have been on service, were also to point out the various circumstances which tended to support, or to abate, the strength and courage of the soldier or the sailor.

hours

hours this state of suffocation, he is comforted with a draught of ale or wine. The purges and sweatings are repeated, according to the grossness of his habit, and from time to time his trainer, (regarding him no otherwise than he would a running horse, under the like discipline,) takes him out, and makes trial of his wind and strength, and does not cease till he has made him as lank as a greyhound, and almost as fleet.

and the great increase of force acquired by the human frame.

A man, even in the best of ordinary 'nealth, becomes giddy and breathless when he strikes; and sick and pale on receiving a few blows. He is thence unable to bear any unusual exertion, and by inference prone to disease. If, by extenuating the fat, emptying in the cellular substance, hardening the muscular fibres, and improving the breath, a man of the ordinary frame may be made to fight for one hour, with the utmost exertion of strength and courage; the inquiry which I have already suggested must be of the highest use. For were this new train of facts regularly laid before professional men, and were they enabled thus to judge of the influence which the methods of these practical philosophers have on regulating the functions of breathing, perspiration and digestion; it would be drawing into the province of science, an art connected most particularly with the means of prolonging life, and hitherto known and practised only by a few insulated individuals, of course imperfectly known, and of too limited use.

These facts are probably of great value to the science of prolonging life.

The art seems to be modern.

I question whether the athletics of old used similar means; whether they were equally successful; whether there ever were, in any climate, age, or country, more hardy or powerful frames than those of our English pugilists. In Cooke's voyage, we are told of the marked inferiority of the English sailors, in wrestling or boxing, to the naked sun-burnt heroes of the South Sea Islands. But an English sailor, though full of spirit and vigour, is as clumsy as a clown, and could not even row against an inhabitant of the Sandwich Islands. An English bricklayer, blacksmith, or drayman, however, who liked the sport, and was practised in balancing and striking, might have challenged the whole of the tawny nation.

Queries.

With a view of collecting such important information, I am very anxious that the following queries should be proposed to those who profess the art of training pugilists, wrestlers, and runners of foot-races, by such intelligent men as have the opportunity of conversing with them.

By

1. By what criterions or ^{they judge of the muscular} strength, or wind, or other ^{abilities of those who seek to put} themselves under training. ^{What is the earliest, and what is} the latest age they would attempt to train? ^{Test of strength &c.?}

2. How they judge of the length of time that may be re- ^{Time required} quired for bringing a man into good plight, vigorous health, ^{to train?} and free breathing; and what period of preparation is usually required for running a match?

3. What purges they use; and in what succession; and by ^{Purges, treat-} what rules do they administer them; and how do they judge ^{ment, their} of their effects? Is the purging only preparatory, or is it ^{object, &c.?} regularly continued? Is it meant, by this process to reduce the plethoric state of the system, (on the idea that there is too great a quantity of blood,) or is it simply designed to put the bowels in the most favourable condition, for easy and good digestion? Is the reducing the actual size of the belly, necessary to more free and perfect breathing *?

4. Is the diet rich or simple; of animal food, or of vegeta- ^{Diet?} ble; in great quantity, or sparing; is it increased gradually, or diminished gradually? What meals have they in the day; and at what hours; one or more; frequent feeding, in small and fixed portions, or full and substantial meals? What kinds of flesh or meat is reckoned the best; whether beef, mutton, veal, pork, lamb, or fowl? Are any kinds of fish allowed? What quality of food is most conducive to strength? What quantity is necessary for maintaining the system in its most perfect state of vigour? Do they feed much in the intermediate days of the purges? Is abstinence required when they take their physic?

5. What kinds of liquors are reckoned best? Whether ^{Liquors?} wine, ale, water, spirits, &c.? Whether given hot or cold; in what quantities; and when ought they to be given?

* The effects of taking up a running horse from idleness and soft pasture, to hard food and regular exercise, is attended with this peculiar effect, that while the animal becomes lank, sleek, and glossy, while he gets fire in his eye, and a new vigour in his limbs, and wind and speed, his belly, (swollen with coarse indigestible food, eaten in great profusion,) is drawn into half its size. May we not then presume from this analogy, that the state of the belly has a remarkable effect upon the wind.

Intention of the perspirations, how excited, &c.?

6. Are the very violent perspirations into which they throw their patients, designed to reduce the system, to extenuate the fat, to lessen that quantity of blood, the excess of which makes us giddy or short breasted; or is it merely designed to produce a new condition of the skin, more favourable to health and muscular vigour; to produce a sharper appetite; a greater demand for food; and a quicker nourishment, or a greater nutrition from a more slender diet? Is the sweat at first produced by exercise, and only continued by the person, when trained, being put between feather beds, and encouraged by drinks; or is it produced by force of sweating drugs, or violent heats, or by continued friction? At what hours are the perspirations brought on? How is the pupil treated when the sweat is over? What becomes of the skin of a fat man, when, by the process, he is reduced in size, and rendered lean? Does it hang loose, or is it tight? Has it any effect upon the bones?

Exercise and treatment?

7. What hours of exercise do they require of their pupils during the day? At what hours do they send them out in the morning? How long do they continue abroad? Are they loaded with clothes after the body is reduced, and becomes limber, and thin and muscular; or only while the sweating process continues? Are they fed before they go abroad, or when they return? What trials are made of their strength? When is a man known to be up to his full strength and breath in training? At what hours do they go to bed? What sleep are they allowed? What indispositions are they subject to during training? Are there any circumstances by which the process may be interrupted; or any circumstances, in consequence of which, it must sometimes be abandoned?

Subsequent effects of training?

8. What is the state of the health, after they give up training? Are they subject to any complaints; and what are they? How long does the acquired excess of strength continue?

What part of training most effectual? Whether it be permanent, temporary, curative

9. It is most interesting to learn, on which part of this process, the purging, the sweating, the exercise, or the feeding, they most depend; and whether it procures a permanent increase of vigour, easily maintained by suitable diet and exercises, or only a temporary excitement, calculated for the particular occasion? Also, whether persons have ever thought of undergoing this process, not for the purpose of running matches, but to recover health; with what success

this

this has been done, and whether it is to be recommended for gout, corpulency, asthma, nervous disorder, or other maladies, as likely to be of service?

These are questions, of the importance of which, those who are best able to answer, may not be fully aware. But nothing which so suddenly changes the powers, and the very form and character of the body, from gross to lean, from weakness to vigorous health, from a breathless and bloated carcase, to one active and untiring, can ever be unimportant, either to the art of physic in general, or to that branch of it more immediately connected with inquiries regarding health and longevity.

The art must be of importance.

The queries to be put regarding jockies, running-horses, or game-cocks, may be to the following effect:

1. *Jockies.*

1. What is the process used in training them, and reducing their weight?
2. What effect has it upon their health and strength?
3. What effect has it upon their mind, in regard to courage, quickness, &c.
4. How long do these effects continue?
5. After being reduced, do they quickly get fat again, or do they continue long in the state to which they were brought?
6. Are jockies, accustomed to be thus treated, healthy and long lived?

Queries respecting Jockies.

2. *Running Horses.*

1. What are the principal objects to be attended to in regard to running-horses? Do their perfections depend upon parentage, and whether most upon the male or the female? Is it necessary that the mare should have gone her full time, to bring a perfect foal? Is the gradual growth of the foal essential? Is there a great difference, in regard to natural constitution, between horses of the same parentage? What kind of form is in general preferred? Do you prefer great or small bones? Which sex is preferable for speed, and which for strength?
2. What is the best age for beginning to train horses for the turf? Are they first put upon grass? What is the effect of soft

Running horses.

soft meat? When should they ^{rather} be put on hard meat? What are the effects thereof? Is it necessary to purge them frequently? Have the purges any tendency to weaken them? What food is reckoned the most nourishing? How often are they fed? What drinks are given them, and how often? Whether hot or cold? Is it necessary to keep their skin perfectly clean, and how? Is it necessary to make them perspire much? What exercise is given them? How is the training completed?

3. After the training is completed, can the perfections thereby obtained be easily kept up? Does the process effect merely a temporary change, or does it last during life? Are running horses as long lived as others, or do they soon wear out?

3. Game-Cocks.

Game-cocks.

1. Does the superiority of game-cocks depend upon parentage? Which is of most importance, the male or the female? Is it of any consequence that the cock should arrive rather gradually at maturity? Is there a great difference, in point of strength and constitution, in game cocks of the same parentage? Do you prefer great or small bones?

2. When do you begin to feed the young cocks? What diet and drink do you give them, and what is the process by which they are brought to the greatest possible height of strength and spirit?

3. When the game-cocks are thus trained, how long do the effects thereof last? Are they temporary or permanent? Do game-cocks thus trained live shorter or longer than others of the same species?

4. What drugs are given to fighting-cocks immediately before the main begins? Is it not usual, by giving them saffron, (or some drug which has the same effect with opium, as used among the Janisaries, or brandy among the French soldiery,) to excite an unnatural and short-lived courage? What are the effects of such drugs? and how do they manage the feeding up to this point, so as to take advantage of this momentary excitement?

*On the Dangers encountered in travelling over Downs, occasioned by Quickfunds, which are frequently found on the Sea Coast; with an Indication of the Means of avoiding them. By M. BIEMONTIER, Inspector-General of Bridges and Roads.**

AFTER heavy and continued rains, there are formed at the edge of the sea-downs, small pools, or collections of water, frequently of several feet in depth. Strong winds dislodge portions of sand from the general mass, and transport them to a distance; which falling in showers on the clayey and sheltered surface of these pools, descend gradually, and remain as it were in equilibrium in the midst of the water, so as to form an infinity of little vaulted cavities. These arches sustain others, which are again surmounted in a similar manner, till at length the mass rises, sometimes to several feet above the level of the water; the surface becomes white and dry, and the snare lies perfectly concealed. Whoever walks over this structure destroys the whole, the arches give way, and the intruder is immersed sometimes to his waist; but his alarm is usually greater than the real danger; for if he were buried even up to the neck, he might easily extricate himself, only by retaining sufficient presence of mind not to struggle, but to move slowly and deliberately; want of attention to this might hazard his destruction.

Quickfunds formed by bodies of sand transported by the wind into pools of water, where they form vaulted cavities till the hollow of the pool is filled up.

The surface is dry and scarcely differs in appearance from the contiguous ground, but it gives way when trod upon.

When the equilibrium of the masses of sand is destroyed, they naturally fall into heaps, and it is only necessary that time should be allowed for this to take place. When this has happened, the person immersed should gently lift up one leg, and remain in that position till the sand has formed a sufficient bottom to support his raised foot; the other leg should then be lifted up with the same precautions; and thus successively, till he rises to the surface. In the mean time, the water which had been confined in the hollows of the sand will have also risen, forming a pond (three or four inches deep) through which the adventurer may pass in perfect safety.

Management to avoid danger.

* *Bibliothèque Physico-Economique, &c. de Senlis, November, 1805, page 186.*

Animals when
immersed use
the same method;

Cows, dogs, and other animals who frequent downs, an chance to fall into these quicksands, either through instinct or experience, make use of this method to regain their freedom provided, however, they be not too deeply immersed to retain the free use of their shoulder joints, otherwise they cannot be extricated without assistance. I experienced this twice in one day; my horse sank to above the breast-leather, and although he was very strong, his efforts to extricate himself were unavailing, till we had removed so much of the sand as impeded the action of those joints.

but they are
feldom caught
in quicksands.

It rarely happens that animals accustomed to live on downs are caught in these snares, which they are aware of, and know how to avoid.

Instance.

I attempted, but in vain, to force another horse with the whip and spur into a quicksand; his owner, who acted as guide, assured me, that I should not succeed, although there was no other indication of the spot than a flat surface, slightly wrinkled. By these marks the traveller may generally detect the concealed pitfall; but he may always avoid them by tracing the footsteps of the cattle, when visible, or by walking a few fathoms above the bottom of the declivity, or on the summit of the down.

Another kind of
quicksand,

Another kind of quicksand is sometimes met with on the sea-shore, between high and low water mark, which it is proper should be here taken notice of. This is sometimes the effect of rain, but more commonly of the sea, when forced by wind and tempest beyond its usual limits, which being generally more elevated than the distant land, the waters thus impelled forward are prevented from returning to their ancient bed; they therefore after forming in a body, drain away through the earth they have inundated, or brought with them, and form excavations beneath, large or small, deep or shallow, according to circumstances.

formed by the
waters drained
through the
earth.

I ought not to omit a singular fact which passed under my own observation, and which seems to prove, (as I have already stated) that animals frequenting these plains, and living near the borders of the sea, employ combined means, acquired undoubtedly by experience, to extricate themselves from these cavities, wherein they must inevitably perish, did they, as it appears natural they should, attempt to escape by recoiling or by flight.

Traversing the plain of Arcachon, after a violent tempest, ^{Singular incident.} which had been accompanied with heavy rains, we thought it prudent to get off our horses and lead them by the bridle. One of the horses who was left to himself, immediately quitted the company; and was retiring from the shore, but being compelled to return by the application of the whip, he went upon the quicksand, which probably he had attempted to avoid by desertion; but the moment he felt the earth giving way, he crouched down, or rather threw himself precipitately on his side. The ground quickly sank beneath and round about him; the water surmounted the sand; the horse was only wetted to the crupper, and we escaped with no other damage than the loss of our stock of bread, which being soaked in the salt water was rendered unfit to be eaten.

It may be received as matter of fact, that a man who ^{Travellers when caught in this snare should throw themselves down.} should experience a similar misfortune, could not do better than to extend himself in the like manner, nearly in the attitude of a swimmer, when he throws himself into the water. It is scarcely necessary to explain the superior advantage of this method; a plate of lead, of some thousands of weight, and several feet in breadth, if cast flat into any liquid body, would reach the bottom no quicker than the fluid could escape to make way for it; if a similar body were to fall upon a quicksand, it would shake every part of it, but would prevent the sand or earth from rising, while the firm surrounding earth would confine it laterally: the ruins of the arched vaults would replace the waters which had been liberated from their subterraneous confinement; solid heaps would then necessarily be formed towards the centre, and the incumbent body would remain at the surface, or at least it would not be swallowed up.

These quicksands are generally denoted by small streams, ^{Quicksands are denoted by rills of water.} below which, when practicable, there is no danger in passing.



*Extract from a Memoir by Messrs. FOURCROY and VAUQUELIN, on the Guano, or Natural Manure, of the small Islands of the South Sea, near the Coast of Peru. Read at the French National Institute, by A. LAUGIER.**

M. Humboldt the first who gave an account of the Guano.

Memoir by Messrs. Fourcroy and Vauquelin on the excrements of birds, suggested the notion that the Guano was derived from the same origin.

AMONG the multitude of subjects worthy the attention of the naturalist, which the philosophical Humboldt observed and collected during his travels, the Guano is not the least considerable, from the interest which it excites. This celebrated naturalist, by making us acquainted with this singular matter, one of the principal resources of agriculture in the countries he visited, has given confirmation to a discovery made by the authors of this memoir, about the time of his return. Reading their memoir on the existence of uric acid in the excrements of birds, it occurred to him that the Guano of the islets on the coast of Peru, which are frequented by great numbers of birds, might possibly be of the same nature. It remained for chemical investigation to examine how far this conjecture was well founded; and Messrs. Fourcroy and Vauquelin undertook the analysis of this matter. The following is the result of their labours, with this view, extracted from the Memoirs of the National Institute.

Before I enter upon a detail of the experiments made upon Guano, in order to ascertain its nature, it may not be irrelevant to the subject to transcribe what M. Humboldt himself says of this substance in a note sent to the authors of this memoir.

Extract from M. Humboldt's note. Guano found on certain small islands,

"The Guano is found in abundance in the South Sea, in the Chinche islands, near Pisco; and also on the more southern coasts and islets of Ilo, Iza, and Arica. The inhabitants of Chancay, who make Guano an object of their commerce, go to and return from the Chinche islands once in 20 days. Each vessel contains from 1500 to 2000 cubic feet. A vanega sells at Chancay for 14 livres, and at Arica for 15 livres, Tournois.

—in beds 50 or 60 feet thick.

"Guano is dug from beds 50 or 60 feet thick; where it is worked like the bog-ore of iron. The islets where it is found

are frequented by a multitude of birds, particularly of the species of *Ardea* and *Phœnicopterus*, who roost there every night: but the excrements of these birds have hardly formed, in three centuries, a layer of four or five lines in depth. Is then the Guano the effect of some convulsion of the globe, like pit-coal and fossil wood? The fertility of the naturally sterile soil of Peru is derived from the Guano, which has become a material article of commerce. Fifty little vessels, called *Guaneras*, are constantly employed in fetching this manure, for the supply of the coast. Its effluvium may be smelled at the distance of a quarter of a league. The sailors accustomed to this smell of ammonia, feel no inconvenience from it; but we could not approach it without being affected with continued fits of sneezing.

The place frequented by vast numbers of birds.

Sterile soil of Peru made fruitful by the Guano.

Vessels employed to collect it. It has a strong odour of ammonia.

“Maize is the particular vegetable for which Guano forms an excellent manure. The Spaniards learned its use of the Indians: If too much be thrown upon the maize, the root is burned and destroyed. Guano is too acidifiable, and is therefore a manure containing hydruret of azote; whilst all other manures are rather hydrurets of carbon.”

Maize particularly benefited by Guano as a manure.

Guano is of a dirty yellow colour, rather insipid to the taste, but possessing a powerful odour, partaking of castor and of valerian. It turns black in the fire, and exhales a white smoke of an ammoniacal smell.

Its appearance.

Its solubility in water, particularly with potash, determined the operators as to the method they should pursue in its analysis. They treated it successively with water, with potash, and with muriatic acid; each of which methods presented many phenomena, as related in the following part of this paper, divested of the particular details of process, which are too extensive for an extract.

Partly soluble in water.

Ten grammes of this matter, after being repeatedly washed with large quantities of boiling water, were reduced to 5.76 grammes. The water had obtained a red colour, which it communicated to paper stained with turnsole.

The solution is acid.

In distillation, the water yielded ammonia during the whole operation. Twenty-four hours afterwards, it had deposited a dirty yellow powder, possessing very little flavour, but with an odour of castor: On the surface was a crystalline pellicle, of the same colour with the deposition.

The water yielded ammonia by distillation, and deposited a yellow powder with a smell of castor.

The

The liquor, filtered and again evaporated, till reduced to 3 grammes, on cooling again, deposited a fawn-coloured powder, similar to the former, but in less quantity.

The powder, and the mother-water, which had held it in solution, were separately examined.

**Examination of
the powder.**

The powder offered the following properties:—It is a concrete and pulverulent substance, of a brilliant crystalline aspect, and of a dull yellow colour. Before the blow-pipe it is consumed entirely away, yielding a slight empyreumatic odour of ammonia and prussic acid. It is very little soluble in cold water; but abundantly so in warm water, to which it communicates its yellowish colour. This solution, though tasteless, strongly reddens the tincture of turnsole, precipitates solutions of acetate of lead, and of nitrate of silver and mercury, in coloured flakes, which are readily and completely redissolved by nitric acid.

This matter instantaneously dissolves in an alkaline ley, which it tinges of a deep brown colour, exhaling a pungent smell of ammonia. Sulphuric acid poured into the concentrated alkaline solution, throws down a very thick whitish precipitate, and disengages a brisk odour, resembling that of weak acetic acid.

**It is an acidu-
lous salt, com-
posed of animal
acid, ammonia,
and lime.**

The learned authors of this memoir conclude from their experiments, that this powder is an acidulous salt, composed of animal acid, ammonia, and a little lime. In fact, very weak nitric acid, wherein this salt had been macerated in order to disengage the acid it contained, from its bases, yielded, on evaporation, copious ammoniacal vapours, by the addition of potash, and unequivocal signs of the presence of lime, by the addition of oxalic acid.

**Analysis of the
powder when
deprived of its
ammonia and
lime.**

When thus deprived of its ammonia and lime, this matter is less coloured and less soluble than before. Its solution in boiling water deposits pretty hard and brilliant crystals, and more deeply reddens turnsole paper. It combines readily, and without any ammoniacal vapour, with potash, from which all the acids again separate it. Heat turns it black; and it burns, without leaving any residuum, with an odour of ammonia and of prussic acid. A neutral combination of it with ammonia will not precipitate the solution of sulphate of alumine, as is done by hoimstic acid.

From these facts it appears evident, 1, that the matter taken up by the boiling water from Guano is an acid, partly saturated with ammonia and a little lime; 2, that this acid is an animal product, because it yields ammonia and prussic acid, when decomposed by fire; 3, that the same acid, according to all the known properties, must be uric acid, similar to that contained in the excrements of aquatic birds; 4, that it forms about one fourth part of the Guano.

The acid of Guano is uric acid, forming about $\frac{1}{4}$ of the whole.

The mother-water which deposited the powder, whose qualities have been just examined, is very acid; potash causes a copious disengagement of ammonia: It contains, therefore, an ammoniacal salt. Nitrate of barytes and of silver announce the presence of muriatic and sulphuric salts; which are precipitated in white flakes by lime-water, and are redissolved, though with difficulty, in muriatic acid.

Analysis of the mother-water which deposited the powder. It contains an ammoniacal salt,

This precipitate caused by lime water, is evidently formed of two salts, both soluble in acids without effervescence; one easily, and without the assistance of heat, the other with difficulty, even with the aid of heat; the former resists calcination, the latter is decomposed by fire, and afterwards dissolves in acids with effervescence. The first is phosphate of lime, the second oxalate of lime.

—and phosphate and oxalate of lime,

Messrs. Fourcroy and Vauquelin wished to separate these two salts, without their undergoing any alteration; and with this view they made use of weak nitric acid, which dissolved the phosphate of lime, and left the oxalate untouched. The latter salt, on being treated with a solution of carbonate of potash, yielded a precipitate that dissolved with effervescence in nitric acid: This solution displayed all the properties of nitrate of lime. The acid separated from the lime was taken up by the potash: in fact, the liquor possessed the characters of oxalate of potash; it precipitated with lime-water, a very divided powder, with sulphate of lime, in flakes, which would not readily unite; and with all the metallic solutions capable of precipitation by oxalic acid. Sulphate of alumine caused no precipitate, as it would have done with borate of potash.

The potash found in the mother-water, after its precipitation by lime-water, and the disengagement of ammonia, caused by the addition of potash to the mother-water, prior to its decomposition by lime-water, sufficiently shew that these two salts saturate the acids contained in the mother-water of

The mother-water contains oxalates, phosphates, sulphates, and muriates of potash, and of ammonia.

Guano; and that the mother-water certainly contains oxalates-phosphates, sulphates, and muriates of potash, and of ammonia.

The Guano left from the first washing, —contains uric acid.

The five grammes and seven-tenths, left after the washing of the ten grammes originally taken for analysis, were treated with caustic potash, which took up eight-tenths. This alkaline solution contained only uric acid, and a small portion of fat matter.

Phosphate of lime, iron, and carbonate of lime;

The 4.9 grammes left by the caustic potash, were treated with muriatic acid: the product was phosphate of lime, iron, and an atom of carbonate of lime.

—and left
A quartzose and ferruginous sand.

After these applications of water, of caustic potash, and of muriatic acid, there remained of the 10 grammes of Guano, only 3.1 grammes of matter, composed of quartzose and ferruginous sands.

Recapitulation of component parts.

From the foregoing interesting analysis, it appears that the manure of the islets of the South Sea is formed of,—

1. Uric acid to the amount of $\frac{1}{4}$ of the whole compound; partly saturated with ammonia and lime:
2. Oxalic acid, partly saturated with ammonia and potash:
3. Phosphoric acid, combined with the same bases and with lime:
4. Small quantities of sulphate and muriates of potash and ammonia:
5. A small portion of fat matter:
6. Sand, partly quartzose and partly ferruginous.

Remarks.

The existence of Guano in places frequented by vast numbers of birds, and the identity of its nature with that of the excrements of aquatic birds, necessarily throw considerable light on the origin of this matter.

The analysis proves how well founded was the ingenious comparison of the learned naturalist, to whom we are indebted for our knowledge of this substance, no less interesting to us than useful to the inhabitants of Peru. It confirms the important discovery made by the researches of Messrs. Fourcroy and Vanquelin. In a word, this analysis possesses the advantage of proving a well-known maxim, that the sciences mutually enrich and enlighten each other with the light they possess; and it affords a new occasion to remark that among the sciences, there are perhaps none which have so immediate and so necessary a connection as Chemistry and Natural History.

XI.

Note on a Varnish for glazing Cups. By M.

PARMENTIER.*

M. BOMPOIX, chief apothecary to the French Military Hospital at Genoa, having sent me some coffee-cups of a remarkable lightness, and glazed with a varnish which is held in great repute, perhaps only because its preparation is kept a secret in that country; I requested him to use his utmost endeavours to procure me the receipt. He obtained it through the medium of one of his pupils, who learned the secret from the artificer at the manufactory, and had made from his prescription a varnish in every respect equal to that in question:

It consisted of lintseed oil 1 $\frac{1}{2}$ lbs.; amber 1lb.; litharge in powder, minium in powder, ceruse in powder, each, 5 oz. Account of very light coffee-cups finely varnished.

Boil the lintseed oil in an unglazed earthen vessel, and tie the litharge, minium, and ceruse in a linen bag, which is to be suspended in the oil whilst boiling, so that it may not touch the bottom of the vessel. When the oil begins to turn brown, take out the bag, and put in a clove of garlic, cleared of the skin; continue the boiling; and when the garlic is dried away, put in another and another, to the amount of six or seven. In the mean time, the amber is to be melted in another unglazed vessel, according to the method hereafter prescribed; and when the oil has been sufficiently boiled, the fused amber is to be poured into it.

To melt the Amber.

Take two ounces of lintseed oil, to soften the amber and to assist its fusion by a very brisk fire, and when the amber is melted, add the lintseed oil, and boil the whole about two minutes. The fluid must then be strained through a coarse cloth, and when cold put into a bottle well corked, to prevent from drying.

Fusion of the amber.

Method of using the Varnish.

Let the piece intended to be varnished be first well polished, and then apply the varnish in the following manner:

Application of the varnish.

* Annales de Chimie, Vol. LVI. p. 254.

Mix lamp black with varnish and a little turpentine, with a hair pencil, and lay one coat on the piece; when this is dry, lay on another, and repeat the process till four coats have been laid on, taking care to let each dry before the application of the next. When the last is dry, put the piece into a stove or oven to complete the drying, and then polish it with pumice and Tripoli powder.

Method of preparing the Piece intended to be varnished.

Manner of
making the
wooden cups.

Make the cups of hazel, alder, or cherry-tree, which are preferable to other woods for this use, because they are porous when perfectly dry, and do not warp. Form them according to fancy, and dry them in an oven. The work must be polished as if it were complete; and afterwards lay on the varnish as already prescribed.

Red varnish.

If it should be wished to give a red ground to the article, mix a little minium, or rather cinabar, with the varnish. Any other colour may in like manner be mixed with it, as may best please the fancy of the operator.

XII.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Action of Heat. By SIR JAMES HALL, Bart. F. R. S. Edinburgh.*

SECTION I.

Ancient Revolutions of the Mineral Kingdom.—Vain Attempts to explain them.—Dependance of Geology on Chemistry.—Importance of the Carbonate of Lime.—Dr BLACK's Discovery of Carbonic Acid subverted the former theories depending on Fire, but gave Birth to that of Dr. HUTTON.—Progress of the Author's Ideas with Regard to that Theory.—Experiments with Heat and Compression, suggested to Dr. HUTTON in 1790.—Undertaken by the Author in 1798.—Speculations on which his Hopes of Success were founded.

Violent revolutions of the surface of the globe.

HOEVER has attended to the structure of rocks and mountains, must be convinced, that our globe has not always existed

* The highly interesting experiments of Sir James Hall upon the effects of heat modified by compression, were communicated to the

discovered in its present state; but that every part of its mass, so far as at least as our observations reach, has been agitated and subverted by the most violent revolutions.

Facts leading to such striking conclusions, however imperfectly observed, could not fail to awaken curiosity, and give rise to a desire of tracing the history, and of investigating the causes, of such stupendous events; and various attempts were made in this way, but with little success; for while discoveries of the utmost importance and accuracy were made in astronomy and natural philosophy, the systems produced by the Geologists were so fanciful and puerile, as scarcely to deserve a serious refutation. Geological systems imperfect.

One principal cause of this failure seems to have lain in the very imperfect state of chemistry, which has only of late years ^{because chemical knowledge was in its infancy.} begun to deserve the name of a science. While chemistry was in its infancy, it was impossible that geology should make any progress; since several of the most important circumstances to be accounted for by this latter science, are admitted on all hands to depend upon principles of the former. The consolidation of loose sand into strata of solid rock; the crystalline arrangement of substances accompanying those strata, and blended with them in various modes, are circumstances of a

the Royal Society of Edinburgh in August 1804, and were transmitted to our Journal by the author in the following month. They appear in Vol. IX. page 98. That concise narrative could not but strongly excite the curiosity of philosophers and geologists, and direct their earnest expectations to a fuller detail. In the last session, June 3, 1805, a very ample communication was made, which has been printed with five quarto plates, very beautifully engraved by Lizars, from designs by Sir James. I cannot but consider it as one of those high marks of approbation, with which the Philosophical Journal has been honoured from time to time, that the author has again directed his attention to this periodical work, as the vehicle through which his discoveries should be more extensively circulated. With this view he has not only favoured me with the memoir as soon as completed, but has liberally taken upon himself the expence of engraving the plates for the Journal in the same superior style. By this means the numbers containing his memoir will be enriched with ten additional plates besides those usually given:—for I shall with great satisfaction follow the steps of the worthy baronet by presenting the additional expences of paper and print to the reader without charge. W. N.

chemical

chemical nature, which all those who have attempted to frame theories of the earth have endeavoured by chemical reasonings to reconcile to their hypotheses.

Fire and water adduced as the agents in two theories.

Fire and water, the only agents in nature by which stony substances are produced, under our observation, were employed by contending sects of geologists, to explain all the phenomena of the mineral kingdom.

Water has little agency on minerals.

But the known properties of water are quite repugnant to the belief of its universal influence, since a very great proportion of the substances under consideration are insoluble, or nearly so, in that fluid; and since, if they were all extremely soluble, the quantity of water which is known to exist, or that could possibly exist in our planet, would be far too small to accomplish the office assigned to it in the Neptunian theory*. On the other hand, the known properties of fire are no less inadequate to the purpose; for, various substances which frequently occur in the mineral kingdom, seem, by their presence, to preclude its supposed agency; since experiment shews, that, in our fires, they are totally changed or destroyed.

Carbon fire does not explain the facts.

Hence both theories were doubtful.

Under such circumstances, the advocates of either element were enabled, very successfully, to refute the opinions of their adversaries, though they could but feebly defend their own: and, owing, perhaps to this mutual power of attack, and for want of any alternative to which the opinions of men could lean, both systems maintained a certain degree of credit; and writers on geology indulged themselves, with a sort of impunity, in a style of unphilosophical reasoning, which would not have been tolerated in other sciences.

Carbonate of lime is of extensive importance,

Of all mineral substances, the *carbonate of lime* is unquestionably the most important in a general view. As limestone or marble, it constitutes a very considerable part of the solid mass of many countries; and, in the form of veins and nodules of spar, pervades every species of stone. Its history is thus interwoven in such a manner with that of the mineral kingdom at large, that the fate of any geological theory must very much depend upon its successful application to the various conditions of this substance. But, till Dr. Black, by his

* *Illustrations of the Huttonian Theory*, by Mr. Professor Playfair: § 430.

discovery of carbonic acid, explained the chemical nature of the carbonate, no rational theory could be formed, of the chemical revolutions which it has undoubtedly undergone.

This discovery was, in the first instance, hostile to the supposed action of fire; for the decomposition of limestone by fire in every common kind being thus proved, it seemed absurd to ascribe to that same agent the formation of limestone, or of any mass containing it. seems not producible by heat.

The contemplation of this difficulty led Dr. Hutton to view the action of fire in a manner peculiar to himself, and thus to form a geological theory, by which, in my opinion, he has furnished the world with the true solution of one of the most interesting problems that has ever engaged the attention of men of science. Dr. Hutton's theory.

He supposed,

I. That heat has acted, at some remote period, on all rocks. That rocks have undergone heat under strong pressure.

II. That during the action of heat, all these rocks (even such as now appear at the surface) lay covered by a superincumbent mass, of great weight and strength.

III. That in consequence of the combined action of heat and pressure, effects were produced different from those of heat on common occasions; in particular, that the carbonate of lime was reduced to a state of fusion, more or less complete, without any calcination.

The essential and characteristic principle of his theory is thus comprised in the word *compression*; and by one bold hypothesis, founded on this principle, he undertook to meet all the objections to the action of fire, and to account for those circumstances in which minerals are found to differ from the usual products of our furnaces.

This system, however, involves so many suppositions, apparently in contradiction to common experience, which meet us on the very threshold, that most men have hitherto been deterred from the investigation of its principles, and only a few individuals have justly appreciated its merits. It was long before I belonged to the latter class; for I must own, that, on reading Dr. Hutton's first geological publication, I was induced to reject his system entirely, and should probably have continued still to do so, with the great majority of the world, but for my habits of intimacy with the author; the vivacity and Singular contrast of the perspicuity of Dr. Hutton's conversation, and the obscurity of his writings.

and perspicuity of whose conversation formed a striking contrast to the obscurity of his writings. I was induced by that charm, and by the numerous original facts which his system had led him to observe, to listen to his arguments, in favour of opinions which I then looked upon as visionary. I thus derived from his conversation the same advantage which the world has lately done from the publication of Mr. Playfair's *Illustrations*; and, experienced the same influence which is now exerted by that work, on the minds of our most eminent men of science.

The author
progress in the
Doctor's theory.

After three years of almost daily warfare with Dr. Hutton, on the subject of his theory, I began to view his fundamental principles with less and less repugnance. There is a period, I believe, in all scientific investigations, when the conjectures of genius cease to appear extravagant; and when we balance the fertility of a principle, in explaining the phenomena of nature, against its improbability as an hypothesis: The partial view which we then obtain of truth, is perhaps the most attractive of any, and most powerfully stimulates the exertions of an active mind. The mist which obscured some objects dissipates by degrees, and allows them to appear in their true colours; at the same time, a distant prospect opens to our view, of scenes unsuspected before.

He proposes ex-
perimental con-
firmation,

Entering now seriously into the train of reasoning followed by Dr. Hutton, I conceived that the chemical effects ascribed by him to compression, ought, in the first place, to be investigated; for, unless some good reason were given us for believing that heat would be modified by pressure, in the manner alledged, it would avail us little to know that they had acted together. He rested his belief of this influence on analogy; and on the satisfactory solution of all the phenomena furnished by this supposition. It occurred to me, however, that this principle was susceptible of being established in a direct manner by experiment, and I urged him to make the attempt; but he always rejected this proposal, on account of the immensity of the natural agents, whose operations he supposed to lie far beyond the reach of our imitation; and he seemed to imagine, that any such attempt must undoubtedly fail, and thus throw discredit on opinions already sufficiently established, as he conceived, on other principles. I was far, however, from

rejected by the
Doctor.

from being convinced by these arguments; for, without being able to prove that any artificial compression to which we could expose the carbonate, would effectually prevent its calcination in our fires, I maintained, that we had as little proof of the contrary, and that the application of a moderate force might possibly perform all that was hypothetically assumed in the Huttonian theory. On the other hand, I considered myself as bound, in practice, to pay deference to his opinion, in a field which he had already so nobly occupied; and abstained, during the remainder of his life, from the prosecution of some experiments with compression, which I had begun in 1790.

In 1798, I resumed the subject with eagerness, being still of opinion that the chemical law which forms the basis of the Huttonian theory, ought, in the first place, to be investigated experimentally; all my subsequent reflections and observations having tended to confirm my idea of the importance of this pursuit, without in any degree rendering me more apprehensive as to the result.

In the arrangement of the following paper, I shall first confine myself to the investigation of the chemical effects of heat and compression, reserving to the concluding part the application of my results to Geology. I shall then appeal to the volcanoes, and shall endeavour to vindicate the laws of action assumed in the Huttonian theory, by shewing, that lavas, previous to their eruptions, are subject to similar laws; and that the volcanoes, by their subterranean and submarine exertions, must produce, in our times, results similar to those ascribed, in that theory, to the former action of fire.

In comparing the Huttonian operations with those of the volcanoes, I shall avail myself of some facts, brought to light in the course of the following investigations, by which a precise limit is assigned to the intensity of the heat, and to the force of compression, required to fulfil the conditions of Dr. Hutton's hypothesis: For, according to him, the power of those agents was very great, but quite indefinite; it was therefore impossible to compare their supposed effects in any precise manner with the phenomena of nature.

My attention was almost exclusively confined to the carbonate of lime, about which I reasoned as follows: The carbonic acid, when uncombined with any other substance, exists naturally.

Experimental investigation undertaken.

Order of the present treatise.

Argument respecting carbonate of lime.

rally in a gaseous form, at the common temperature of our atmosphere; but when in union with lime, its volatility is repressed, in that same temperature, by the chemical force of the earthy substance, which retains it in a solid form. When the temperature is raised to a full red-heat, the acid acquires a volatility by which that force is overcome, it escapes from the lime, and assumes its gaseous form. It is evident, that were the attractive force of the lime increased, or the volatility of the acid diminished by any means, the compound would be enabled to bear a higher heat without decomposition, than it can in the present state of things. Now, pressure must produce an effect of this kind; for when a mechanical force opposes the expansion of the acid, its volatility must, to a certain degree, be diminished. Under pressure, then, the carbonate may be expected to remain unchanged in a heat, by which, in the open air, it would have been calcined. But experiment alone can teach us what compressing force is requisite to enable it to resist any given elevation of temperature; and what is to be the result of such an operation. Some of the compounds of lime with acids are fusible, others refractory; the carbonate, when constrained by pressure to endure a proper heat, may be as fusible as the muriate.

Pressure must oppose the expansion and escape of the acid, and enable it to support a stronger heat.

Probability that the carbonate might not be of difficult fusion.

Facts which indicate its melting heat.

One circumstance, derived from the Huttonian Theory, induced me to hope, that the carbonate was easily fusible, and indicated a precise point, under which that fusion ought to be expected. Nothing is more common than to meet with nodules of calcareous spar inclosed in whinstone; and we suppose, according to the Huttonian theory, that the whin and the spar had been liquid together; the two fluids keeping separate, like oil and water. It is natural, at the junction of these two, to look for indications of their relative fusibilities; and we find, accordingly, that the termination of the spar is generally globular and smooth; which seems to prove, that, when the whin became solid, the spar was still in a liquid state; for had the spar congealed first, the tendency which it shews, on all occasions of freedom, to shoot out into prominent crystals, would have made it dart into the liquid whin, according to the peculiar forms of its crystallization; as has happened with the various substances contained in whin, much more refractory than itself, namely, augite, feldspar, &c.; all of which having congealed in the liquid whin, have assumed their peculiar forms

forms with perfect regularity. From this I concluded, that when the whin congealed, which must have happened about 28° or 30° of Wedgwood, the spar was still liquid. I therefore expected, if I could compel the carbonate to bear a heat of 28° without decomposition, that it would enter into fusion. The sequel will shew that this conjecture was not without foundation.

I shall now enter upon the description of those experiments, The experiments introduced. the result of which I had the honour to lay before this Society on the 30th of August last (1794); fully aware how difficult it is, in giving an account of above five hundred experiments, all tending to one point, but differing much from each other in various particulars, to steer between the opposite faults of prolixity and barrenness. My object shall be to describe, as shortly as possible, all the methods followed, so as to enable any chemist to repeat the experiments; and to dwell particularly on such circumstances only as seem to lead to conclusions of importance.

The result being already known, I consider the account I am about to give of the execution of these experiments, as addressed to those who take a particular interest in the progress of chemical operations: in the eyes of such gentlemen, I trust, that none of the details into which I must enter, will appear superfluous.

SECTION II.

Principle of Execution upon which the following Experiments were conducted.—Experiments with Gun-Barrels filled with baked Clay, and welded at the Muzzle.—Method with the fusible Metal.—Remarkable Effects of its Expansion.—Necessity of introducing Air.—Results obtained.

When I first undertook to make experiments with heat acting under compression, I employed myself in contriving various devices of screws, of bolts, and of lids, so adjusted, I hoped, as to confine all elastic substances; and perhaps some of them might have answered. But I laid aside all such devices, in favour of one which occurred to me in January 1798; which, by its simplicity, was of easy application in all cases, and accomplished all that could be done by any device, since it secured perfect strength and tightness to the utmost that the vessels employed could bear, whether formed of metallic or earthy substance. The device depends upon the following general view: If we take a hollow tube or barrel (AD Pl. ix. fig. The author's early contrivances for confining elastic substances at high temperatures.

The method adopted ultimately was to include the subject in an iron barrel, and close the aperture by fusion:

fig. 1.) * closed at one end, and open at the other, of one foot or more in length; it is evident, that by introducing one end into a furnace, we can apply to it as great heat as art can produce, while the other end is kept cool, or, if necessary, exposed to extreme cold. If, then, the substance which we mean to subject to the combined action of heat and pressure be introduced into the breech or closed end of the barrel (CD), and if the middle part be filled with some refractory substance, leaving a small empty space at the muzzle (AB), we can apply heat to the muzzle, while the breech containing the subject of experiment, is kept cool, and thus close the barrel by any of the numerous modes which heat affords, from the welding of iron to the melting of sealing-wax. Things being then reversed, and the breech put into the furnace, a heat of any required intensity may be applied to the subject of experiment, now in a state of constraint.

First experiment with the muzzle plugged and welded,

My first application of this scheme was carried on with a common gun-barrel, cut off at the touch-hole, and welded very strongly at the breech by means of a plug of iron. Into it I introduced the carbonate, previously rammed into a cartridge of paper or pasteboard, in order to protect it from the iron, by which, in some former trials, the subject of experiment had been contaminated throughout during the action of heat. I then rammed the rest of the barrel full of pounded clay previously baked in a strong heat, and I had the muzzle closed like the breech, by a plug of iron welded upon it in a common forge; the rest of the barrel being kept cold during this operation, by means of wet cloths. The breech of the barrel was then put horizontally into a common muffle, heated to about 25° of Wedgwood. To the muzzle a rope was fixed, in such a manner, that the barrel could be withdrawn without danger from an explosion *. I likewise, about this time closed the muzzle

in another instance soldered.

* This plate will be given in No. 54, being the supplement to the present volume.

† On one occasion, the importance of this precaution was strongly felt. Having inadvertently introduced a considerable quantity of moisture into a welded barrel, an explosion took place, before the heat had risen to redness, by which, part of the barrel was spread out to a flat plate, and the furnace was blown to pieces. Dr. Kennedy, who happened to be present on this occasion, observed, that notwithstanding this accident, the time might come when we should employ water in these experiments to assist the force

of the barrel, by means of a plug, fixed by solder only; which method had this peculiar advantage, that I could shut and open the barrel without having recourse to a workman. In these trials, though many barrels yielded to the expansive force, others resisted it, and afforded some results that were in the highest degree encouraging, and even satisfactory, could they have been obtained with certainty on repetition of the process. In many of them, chalk, or common limestone previously pulverised, was agglutinated into a stony mass, which required a smart blow of a hammer to break it, and felt under the knife like a common limestone; at the same time, the substance, when thrown into nitric acid, dissolved entirely with violent effervescence.

Satisfactory results.

In one of these experiments, owing to the action of heat on the cartridge of paper, the baked clay, which had been used to fill the barrel, was stained black throughout, to the distance of two-thirds of the length of the barrel from its breech. This circumstance is of importance, by shewing, that though all is tight at the muzzle, a protrusion may take place along the barrel, greatly to the detriment of complete compression: and, at the same time, it illustrates what has happened occasionally in nature, where the bituminous matter seems to have been driven by superior local heat, from one part of a coaly bed, though retained in others, under the same compression. The bitumen so driven off being found, in other cases, to pervade and tinge beds of slate and of sandstone.

Volatile matter may be driven from one to another part of a closed barrel.

I was employed in this pursuit in spring 1800, when an event of importance interrupted my experiments for about a year. But I resumed them in March 1801, with many new plans of execution, and with considerable addition to my apparatus.

In the course of my first trials, the following mode of execution had occurred to me, which I now began to put in practice. It is well known to chemists, that a certain composition of

Experiments in which the fusible metal was used as the plug.

force of compression. I have since made great use of this valuable suggestion: but he scarcely lived, alas! to see its application; for my first success in this way took place during his last illness.—I have been exposed to no risk in any other experiment with iron barrels; matters being so arranged, that the strain against them has only commenced in a red heat, in which the metal has been so far softened, as to yield by laceration like a piece of leather.

different

Advantages of
this method.

different metals*, produces a substance so fusible, as to melt in the heat of boiling-water. I conceived that great advantage, both in point of accuracy and dispatch, might be gained in these experiments, by substituting this metal for the baked clay above mentioned: That after introducing the carbonate into the breech of the barrel, the fusible metal, in a liquid state, might be poured in, so as to fill the barrel to its brim: That when the metal had cooled and become solid, the breech might, as before, be introduced into a muffle, and exposed to any required heat, while the muzzle was carefully kept cold. In this manner, no part of the fusible metal being melted but what lay at the breech, the rest, continuing in a solid state, would effectually confine the carbonic acid: That after the action of strong heat had ceased, and after all had been allowed to cool completely, the fusible metal might be removed entirely from the barrel, by means of a heat little above that of boiling water, and far too low to occasion any decomposition of the carbonate by calcination, though acting upon it in freedom; and then, that the subject of experiment might, as before, be taken out of the barrel.

This scheme, with various modifications and additions, which practice has suggested, forms the basis of most of the following methods.

A striking phenomenon. When the barrel was completely filled with fusible metal only, and the closed end of the iron exposed to heat in a muffle, the greater expansion of the fluid forced it through the texture of the iron in very fine wire resembling wool.

In the first trial, a striking phenomenon occurred, which gave rise to the most important of these modifications. Having filled a gun-barrel with the fusible metal, without any carbonate; and having placed the breech in a muffle, I was surprised to see, as the heat approached to redness, the liquid metal exuding through the iron in innumerable minute drops, dispersed all round the barrel. As the heat advanced, this exudation increased, till at last the metal flowed out in continued streams, and the barrel was quite destroyed. On several occasions of the same kind, the fusible metal, being forced through some very minute aperture in the barrel, spouted from it to the distance of several yards, depositing upon any substance opposed to the stream, a beautiful assemblage of fine wire, exactly in the form of wool. I immediately understood that the phenomenon was produced by the superior expansion of the liquid over the solid metal, in con-

* Eight parts of bismuth, five of lead, and three of tin.

sequence of which, the fusible metal was driven through the iron as water was driven through silver * by mechanical percussion in the Florentine experiment. It occurred to me, that this might be prevented by confining along with the fusible metal a small quantity of air, which, by yielding a little to the expansion of the liquid, would save the barrel. This remedy was found to answer completely, and was applied, in all the experiments made at this time †.

Remedy. A small quantity of air was left in the barrel.

I now proposed, in order to keep the carbonate clean, to inclose it in a small vessel; and to obviate the difficulty of removing the result at the conclusion of the experiment, I further proposed to connect that vessel with an iron ramrod, longer than the barrel, by which it could be introduced or withdrawn at pleasure.

The carbonate was included in a small separate vessel.

* *Effays of Natural Experiments made in the Academie del Cimento*, translated by Waller, London, 1684, page 117. The same in Musschenbroek's Latin Translation, Ludg. Bat. 1731, p. 63.

† I found it a matter of much difficulty to ascertain the proper quantity of air which ought to be thus inclosed. When the quantity was too great, the result was injured by diminution of elasticity, as I shall have occasion fully to shew hereafter. When too small, or when, by any accident, the whole of this included air was allowed to escape, the barrel was destroyed.

I hoped to ascertain the bulk of air necessary to give liberty to the expansion of the liquid metal, by measuring the actual quantity expelled by known heats from an open barrel filled with it. But I was surprised to find, that the quantity thus discharged, exceeded in bulk that of the air which, in the same heats, I had confined along with the carbonate and fusible metal in many successful experiments. As the expansion of the liquid does not seem capable of sensible diminution by an opposing force, this fact can only be accounted for by a distension of the barrel. In these experiments, then, the expansive force of the carbonic acid, of the included air, and of the fusible metal, acted in combination against the barrel, and were yielded to in part by the distension of the barrel, and by the condensation of the included air. My object was to increase the force of this mutual action, by diminishing the quantity of air, and by other devices to be mentioned hereafter. Where so many forces were concerned, the laws of whose variations were unknown, much precision could not be expected, nor is it wonderful, that in attempting to carry the compressing force to the utmost, I should have destroyed barrels innumerable.

A small

Description of
this apparatus

A small tube of glass, * or of Reaumur's porcelain, about a quarter of an inch in diameter, and one or two inches in length, (fig. 2, A) was half filled with pounded carbonate of lime, rammed as hard as possible; the other half of the tube being filled with pounded silice, or with whatever occurred as most likely to prevent the intrusion of the fusible metal in its liquid and penetrating state. This tube so filled, was placed in a frame or cradle of iron (*d f k h*, figs. 3, 4, 5, and 6), fixed to the end (*m*) of a ram-rod (*n n*). The cradle was from six to three inches in length, and as much in diameter as a gun-barrel would admit with ease. It was composed of two circular plates of iron, (*d e f g* and *h i k l*, seen edge-wise in the figures), placed at right-angles to the ram-rod, one of these plates (*d e f g*) being fixed to it by the centre (*m*). These plates were connected together by four ribs or flattened wires of iron (*d h*, *e i*, *f k*, and *g l*), which formed the cradle into which the tube (A), containing the carbonate, was introduced by thrusting the adjacent ribs asunder. Along with the tube just mentioned, was introduced another tube (B), of iron or porcelain, filled only with air. Likewise, in the cradle, a pyrometer † piece (C) was placed in contact with (A) the tube con-

* I have since constantly used tubes of common porcelain, finding glass much too fusible for this purpose.

† The pyrometer-pieces used in these experiments were made, under my own eye. Necessity compelled me to undertake this laborious and difficult work, in which I have already so far succeeded as to obtain a set of pieces, which, though far from complete, answer my purpose tolerably well. I had lately an opportunity of comparing my set with that of Mr. Wedgwood, at various temperatures, in furnaces of great size and steadiness. The result has proved, that my pieces agree as well with each other as his, though with my set each temperature is indicated by a different degree of the scale. I have thus been enabled to construct a table, by which my observations have been corrected, so that the temperatures mentioned in this paper are such as would have been indicated by Mr. Wedgwood's pieces. By Mr. Wedgwood's pieces, I mean those of the only set which has been sold to the public, and by which the melting heat of pure silver is indicated at the 22d degree. I am well aware, that the late Mr. Wedgwood, in his Table of Fusibilities, has stated that fusion as taking place

containing the carbonate. These articles generally occupied the whole cradle; when any space remained, it was filled up by a piece of chalk dressed for the purpose. (Fig. 4, represents the cradle filled, as just described).

Things being thus prepared, the gun-barrel, placed erect with its muzzle upwards, was half filled with the liquid fusible metal. The cradle was then introduced into the barrel, and plunged to the bottom of the liquid, so that the carbonate was placed very near the breech, (as represented in fig. 5, the fusible metal standing at *o*). The air-tube (B) being placed so as to enter the liquid with its muzzle downwards, retained great part of the air it originally contained, though some of it might be driven off by the heat, so as to escape through the liquid. The metal being now allowed to cool, and to fix round the cradle and ramrod, the air remaining in the air-tube was effectually confined, and all was held fast. The barrel being then filled to the brim with fusible metal, the apparatus was ready for the application of heat to the breech, (as shewn in fig. 6.) Plate X.

Method of using the same,

In the experiments made at this time, I used a square brick furnace, (figs. 7 and 8, having a muffle (*r s*) traversing it horizontally and open at both ends. This muffle being supported in the middle by a very slender prop, was exposed to fire from below, as well as all round. The barrel was placed in the muffle, with its breech in the hottest part, and the end next the muzzle projecting beyond the furnace, and surrounded with cloths which were drenched with water from time to time. (This arrangement is shewn in fig. 7.) In this situation, the fusible metal surrounding the cradle being melted, the air contained in the air-tube would of course seek the highest position, and its first place in the air-tube would be occupied by fusible metal. (In fig. 6, the new position of the air is shewn at *p q*).

the furnace and muffle arrangement, &c.

At the conclusion of the experiment, the metal was generally removed by placing the barrel in the transverse muffle, with its muzzle pointing a little downwards, and so that the heat was applied first to the muzzle, and then to the rest of the barrel in succession. (This operation is shewn in fig. 8.) In

Method of disengaging the contents after experiment.

place at the 28th degree; but I am convinced his observations must have been made with some set different from that which was afterwards sold.

some of the first of these experiments, I loosened the cradle, by plunging the barrel into heated brine, or a strong solution of muriate of lime; which last bears a temperature of 250° of Fahrenheit before it boils. For this purpose, I used a pan three inches in diameter, and three feet deep, having a flat basin at top to receive the liquid when it boiled over. The method answered, but was troublesome, and I laid it aside. I have had occasion, lately, however, to resume it in some experiments in which it was of consequence to open the barrel with the least possible heat*.

By these methods I made a great number of experiments, with results that were highly interesting in that stage of the business, though their importance is so much diminished by the subsequent progress of the investigation, that I think it proper to mention but very few of them.

Calcareous spar
converted into
hard dense
marble by heat
of 35° Wedg-
wood's.

On the 31st of March, 1801, I rammed forty grains of pounded chalk into a tube of green bottle-glass, and placed it in the cradle as above described. A pyrometer in the muffle along with the barrel indicated 33° . The barrel was exposed to heat during seventeen or eighteen minutes. On withdrawing the cradle, the carbonate was found in one solid mass, which had visibly shrunk in bulk, the space thus left within the tube being accurately filled with metal, which plated the carbonate all over without penetrating it in the least, so that the metal was easily removed. The weight was reduced from forty to thirty-six grains. The substance was very hard, and resisted the knife better than any result of the kind previously obtained; its fracture was crystalline, bearing a resemblance to white saline marble; and its thin edges had a decided semitransparency, a circumstance first observed in this result.

Calcareous spar
rendered crystal-
line with thim-
boidal fracture
by heat 23° .

On the 3d of March of the same year, I made a similar experiment, in which a pyrometer-piece was placed within

* In many of the following experiments, lead was used in place of the fusible metal, and often with success; but I lost many good results in this way: for the heat required to liquefy the lead approaches so near to redness, that it is difficult to disengage the cradle without applying a temperature by which the carbonate is injured. I have found it answer well, to surround the cradle and a few inches of the tube with fusible metal, and to fill the rest of the barrel with lead.

the barrel, and another in the muffle; they agreed in indicating 23° . The inner tube, which was of Reaumur's porcelain, contained eighty grains of pounded chalk. The carbonate was found, after the experiment, to have lost $3\frac{1}{2}$ grains. A thin rim, less than the 20th of an inch in thickness, of whitish matter, appeared on the outside of the mass. In other respects, the carbonate was in a very perfect state; it was of a yellowish colour, and had a decided semitransparency and saline fracture. But what renders this result of the greatest value, is, that on breaking the mass, a space of more than the tenth of an inch square, was found to be completely crystallized, having acquired the rhomboidal fracture of calcareous spar. It was white and opaque, and presented to the view three sets of parallel plates which are seen under three different angles. This substance, owing to partial calcination and subsequent absorption of moisture, had lost all appearance of its remarkable properties in some weeks after its production; but this appearance has since been restored, by a fresh fracture, and the specimen is now well preserved by being hermetically inclosed,

(To be continued.)

XIII.

On the Use of the Sutures in the Skulls of Animals. By
Mr. B. GIBSON *.

THE full use of the singular junction of the bones of the skull, which is called future, has, from the earliest periods of anatomy and surgery, attracted the attention and eluded the researches of the physiologist. To this remarkable feature in osteogony, in a great measure peculiar to a certain period of life, many uses have been attributed. Some of these are totally erroneous; such as that for allowing the transpiration of moisture, to keep the brain cool and fit for thinking; for giving a more strict adhesion of the *dura mater* to the inner surface of the skull; for admitting a more free communication by blood-vessels between the external and internal parts of the head; or for affording interstices, that the bones may be

*Conjectures on
the use of the
futures in the
skulls of animals.*

pushed afunder by the growth of the brain, lest that organ should be cramped in its growth, in consequence of the comparatively slow growth of the bones of the skull.

Other supposed
uses.

Other uses attributed to the sutures are merely slight advantages derived from their structure, which are enjoyed in early infancy, or till adult life, but gradually cease after that period. Thus at the time of birth the loose union of the bones of the skull accommodates the shape of the head to the figure of the different parts of the cavity through which it passes. At adult age, when the sutures are fully formed, they may occasionally check the progress (if I may be allowed the expression) of a fracture nearly spent;—or vibrations, communicated to the bones of the skull, will be propagated with less force to the brain, in consequence of the bones being separated at the sutures. It is, however, abundantly evident, that these are not the main purposes for which the sutures are formed; otherwise they would not begin to be obliterated at a period of life when they would perform these offices more usefully than ever. Consistent with this remark we shall find, that the true purpose for which they are formed, and the particular process with which they are connected, is fully completed before their obliteration takes place.

The cartilage
between bones
destined to be
united, disap-
pears at last.

When we take a view of the mode of junction between many bones, and parts of bones in the human body, which do not admit of motion, we find that with little exception they all agree in this particular; that sooner or later the cartilage or periosteum which once was interposed is obliterated, and these different portions, or entire bones, coalesce.

Instances of the
system in
bones.

The separate portions, which originally compose the vertebræ, are early in thus uniting; after these the sides of the lower jaw; at a later period the epiphysis of a cylindrical bone is united to its body; and still later the bones of the skull usually coalesce, and the sutures are obliterated. Other bones, as those of the face, which have no motion and sustain little weight, are irregular in this respect; sometimes uniting, but generally remaining distinct, to the end of a long life.

Manner in
which the
osseous system
is completed,
&c.

The original formation of the osseous system in several distinct pieces, respects principally its speedy ossification at an early period of life, and its future convenient extension, till it has arrived at its full growth; and we may consider it as
a general

a general principle, that where two parts of *one* bone are separated from each other by an intervening cartilage, or *two* distinct bones merely by periosteum, at that part osseous materials are added to increase their length or extend their superficies. This we shall find takes place, whether the junction be effected by comparatively smooth surfaces, as between the body of a bone and its epiphysis; or, between the bones of the skull by jagged sutures. Hence it appears that the bones of the body generally are increased in length or extent, not by a uniform extension of the whole substance, but by an addition of bony matter in some particular part.

Thus the body of a cylindrical bone is lengthened by addition to each end. This we might conclude would be the case, from considering the part in which its ossification commences: as this commences in a middle point and proceeds to each extremity, it is natural to suppose that its growth still goes on in the same direction, or continues at the extremities. That this is the case we know, not by reasoning alone, but by a direct experiment. Mr. Hunter sunk two small pieces of lead in the middle of the tibia, or shin bone of a pig, and measured accurately the distance between them; on examining the animal some time afterwards, it appeared, that though the bone had increased considerably in length, the pieces of lead still remained at the same distance from each other that they were before. From this experiment we learn, that a cylindrical bone is not extended in its middle, but is lengthened by addition to its extremities, where the body of the bone is joined to its epiphysis; the chief intention of the epiphysis being to allow the intervention of a vascular organ, which may conveniently deposit bony materials, without interfering with the joint itself.

Cylindrical bones are lengthened by additions at each end.

As cylindrical bones are lengthened at their extreme parts, we are led by analogy to conclude, that the same general plan is pursued in the extension of the flat bones of the body: and although we have no direct experiment by which this has been proved, there are circumstances which leave little doubt but they are extended by addition to their edges. Thus to take the parietal bone as an example; as ossification begins in a central point and extends towards the circumference, it is probable that to the completion of the process, it continues to go on in the same direction; and the same circumstance taking place

The same process appears to take place at the edges of flat bones.

Instance in the
Skull.

place in every bone of the cranium, it is probable that even after the whole of the brain is incased in bone, the addition is still made at the edge of each, and that the general enlargement originates where they are all mutually joined by the sutures. Of this process I had a very striking illustration some years ago. In a young subject, from what cause I know not, the deposition of osseous matter had been suddenly increased a short time before death. It was in different stages of progress, but had taken place in all the bones of the body which I preserved; in some partially, in others generally. In all, the new osseous matter was elevated above the level of the bone upon which it was placed. In some parts of the parietal bones it was only in its commencement, and put on the appearance of a net-work, similar to that which may be observed in the same bones at an early period of their formation. In other parts the meshes of the net-work were more or less filled up; in others again completely, so as to put on the uniform appearance of solid bone. The same reticulated appearance was evident on the edges of all the bones of the skull, where they form the sutures, and at the extremities of the cylindrical bones, between the body and epiphysis. The same appearance of increased deposition was seen on the surface of the cylindrical bones, with this difference, that the meshes were not circular, but oblong squares; so as to put on more of the striated appearance. In some parts, the newly secreted bone was easily separable from the general mass, and formed a thin layer externally, affording one of the best proofs I have met with, of the increase of cylindrical bones in thickness by deposition externally, whilst a corresponding internal absorption goes on. From the striking similarity of appearance on the surfaces and edges of the bones, we may safely conclude, that the same process of deposition was going on in both, and may thence infer, that the bones of the skull are increased in extent by the deposition of osseous matter at their edges, or where they are joined to each other by future. This fact points out to us, in a great measure, the real use of this peculiar mode of junction.

The serrated
edges give firm-
ness, &c.

In order that the bones of the skull may be increased in extent, it is necessary that they should be retained at a certain distance from each other; that the periosteum with its vessels

may pass down between them, free from compression and secrete the osseous matter. At the same time, the thin bones composing the upper part of the skull, resting as an arch upon its basis, must be united together so firmly, as not to be separated by common degrees of violence. For this purpose, projecting points from the external surface of each bone, are reciprocally received into corresponding niches; which only penetrate through one half of the thickness of the skull, and form an irregular kind of dovetailing.

Two advantages arise from this structure, being superficial, and confined to the external table of the skull. The projecting points from each side, resting upon the solid surface of the internal table of the opposite bone, can resist more effectually any violence, which might tend to force the bones inwards; and the internal part of the skull presents, by this means, a smooth surface to the coverings of the brain; for internally no appearance of a jagged suture is seen.

From this view of the subject we see, that the futures of the human skull, by their peculiar formation, at once unite the bones together, and so far separate them, as to allow the interposition of a vascular organ by which their superficies is gradually increased to its greatest extent *. This explanation of

Thus the futures unite the bones, and admit the vascular organ requisite for their growth.

* Since this paper was written in the year 1800, I have found, that a similar opinion was published by Professor Soemmerring in 1794, in his valuable work, "De corporis humani fabrica." To him, therefore, any credit which may belong to the primary suggestion of this use of the futures is due. As his opinion, however, has been little noticed by anatomists generally, and is placed in a clearer point of view by the facts which suggested this further explanation of it to me, it has not been thought improper to give this essay a place in these Memoirs. But whilst the reader will see, by the following quotation, the near resemblance between the opinion of Professor Soemmerring and that which I have brought forward, I hope the character of plagiarist or compiler will not be attributed to me.

"*Usque horum sic sese habentium terminorum ossa cranii inter bene liquet.*

"Incrementum ambitus calvarie levant, ni enim inter ossa capitae mox post partum futurae interponerentur, hæc crescere non possent, nisi aliâ ratione natura rem institueret. Tali igitur modo incrementum calvarie cum incremento reliquorum ossium convenit; initio enim futuri, vel potius lineis cartilagineis ossa

of the use of sutures comprehends and accounts for those concomitant circumstances, which were considered by older anatomists as their real use; and, as far as I can see, is not contradicted by any fact connected with them.

Other remark
and inferences

It it be asked, for instance, why at the sutures there is a stronger adhesion of the *dura mater* internally and *periosteum* externally than in other parts of the skull? the answer is, that these membranes with their vessels are continued into the sutures, to form conjointly the secretory organ, by which the bones are extended.

If it be asked, why there is a greater vascularity or an appearance of blood-vessels passing through the sutures? it is perfectly consistent with this opinion to answer, that the increase of blood goes to this secretory organ, for the purpose of the extension of the bones.

Why the fu-
tures are obli-
terated, &c.

The explanation here offered accounts also for the general obliteration of the sutures after a certain period of life; for the bones having then arrived at their full size, the organ for the secretion of osseous matter is no longer needed; it shrinks and is absorbed, and the bones gradually coalesce; by which a further advantage is derived, that of an accession of strength to the cranium at large.

his locis conglutinantur, verum tamen non nisi in embrionibus ad fontículos, ut aiunt, hæc linea notabili latitudine, observatur. Ossibus enim capitis hic locorum cerebro crescente, placide quasi deductus, cartilago augetur, latior evasura, nisi pristina pars simul in os mutaretur, inde ossa calvariae, eodem modo, quo ossa longa deductis epiphyseibus, vel quod unum idemque est, marginibus crescere, liquet, etsi in ossibus, longis futura epiphyse inter et diaphysin non crispetur.

“Quo junior igitur infans, eo minus crispa et implexa futura, vel ut rectius loquar, linea cartilaginosa angusta, ossa jungens, observatur. Quum vero aucta ætate ossa, crescente cerebro, deducuntur, eorumque, crassitudo adposita cum internæ, tum externæ potissimum tabulæ, (internæ enim incrementum citius, abesse, non videtur) massa ossea, augetur, non potest non esse, quin hæc crispa futuræ forma, quum quidem nasci cœpit, externâ in superficie tandem, augeatur, donec tandem ipsa ea quam maxime impediât, quo minus cerebrum calvariam ulterius deducere possit, quod pubertatis tempore accidit. Rarissime hæc ossificatio ad ætatem virilem usque, detinetur.”—Soemmerring de corporis Humani Fabrica, page 212.

If any additional argument be necessary in support of this opinion, I may also notice the striking analogy which subsists between the separation of one bone of the skull from another by a suture; and that separation which exists between the body of a cylindrical bone and its epiphysis. They each remain only for a certain length of time; each allows the interposition of a secretory organ; and both begin to be obliterated when the bones with which they are connected have completed their growth, and their continuance is no longer necessary.

XIV.

On the Reproduction of Buds. By THOMAS ANDREW KNIGHT,
Esq. F. R. S.*

MY DEAR SIR,

EVERY tree in the ordinary course of its growth generates, in each season, those buds which expand in the succeeding spring; and the buds thus generated, contain, in many instances, the whole of the leaves which appear in the following summer. But if these buds be destroyed during the winter or early part of the spring, other buds, in many species of trees, are generated, which in every respect perform the office of those which previously existed, except that they never afford fruit or blossoms. This reproduction of buds has not escaped the notice of naturalists; but it does not appear to have been ascertained by them from which, amongst the various substances of the tree, the buds derive their origin.

Du Hamel conceived that reproduced buds sprang from pre-organized germs; but the existence of such germs has not, in any instance, been proved, and it is well known that the roots, and trunk, and branches of many species of trees will, under proper management, afford buds from every part of their surfaces; and therefore, if this hypothesis be well founded, many millions of such germs must be annually generated in every large tree; not one of which in the ordinary course of nature will come into action: and as nature, amidst all its exu-

If the generated buds of a season be destroyed, others are produced.

Du Hamel's opinion that these last are from pre-organized germs.

Objection.

* Phil. Transf. 1805.

herance, does not abound in useless productions, the opinions of this illustrious physiologist are, in this case, probably erroneous.

Supposition that they are afforded by the bark.

Other naturalists have supposed the buds, when reproduced, to spring from the plexus of vessels which constitutes the internal bark; and this opinion is, I believe, much entertained by modern botanists; it nevertheless appears to be unfounded, as the facts I shall proceed to state will evince.

Instance to the contrary in sea-calc. Internal buds.

If the fruit-stalks of the sea-calc. (*crambe maritima*) be cut off near the ground in the spring, the medullary substance, within that part of the stalk which remains attached to the root, decays; and a cup is thus formed in which water collects in the succeeding winter. The sides of this cup consist of a woody substance, which in its texture and office, and mode of generation, agrees perfectly with the alburnum of trees; and I conceive it to be as perfect alburnum, as the white wood of the oak or elm: and from the interior part of this substance, within the cup, I have frequently observed new buds to be generated in the ensuing spring. It is sufficiently obvious that the buds in this case do not spring from the bark; but it is not equally evident that they might not have sprung from some remains of the medulla.

Potatoes afford buds at the cut surface,

In the autumn of 1802, I discovered that the potatoe possessed a similar power of reproducing its buds. Some plants of this species had been set, rather late in the preceding spring, in very dry ground, where, through want of moisture, they vegetated very feebly; and the portions of the old roots remained sound and entire till the succeeding autumn. Being then moistened by rain, many small tubers were generated on the surfaces made by the knife in dividing the roots into cuttings; and the buds of these, in many instances, elongated into runners, which gave existence to other tubers, some of which I had the pleasure to send to you.

—and therefore not from the bark.

I have in a former paper remarked, that the potatoe consists of four distinct substances, the epidermis, the true skin, the bark, and its internal substance, which, from its mode of formation, and subsequent office, I have supposed to be alburnous: there is also in the young tubes a transparent line through the centre, which is probably its medulla. The buds and runners sprang from the substance which I conceive to be the alburnum of the root, and neither from the central part of it, nor from the

the surface in contact with the bark. It must, however, be admitted, that the internal substance of the potatoe corresponds more nearly with our ideas of a medullary than of an alburnous substance, and therefore this, with the preceding facts, is adduced to prove only that the reproduced buds of these plants are not generated by the cortical substance of the root: and I shall proceed to relate some experiments on the apple, and pear, and plum-tree, which I conceive to prove that the reproduced buds of those plants do not spring from the medulla.

Having raised from seeds a very considerable number of plants of each of these species in 1802, I partly disengaged them from the soil in the autumn, by digging round each plant, which was then raised about two inches above its former level. A part of the mould was then removed, and the plants were cut off about an inch below the points where the seed-leaves formerly grew; and a portion of the root, about an inch long, without any bud upon it, remained exposed to the air and light. In the beginning of April, I observed many small elevated points on the bark of these roots, and, removing the whole of the cortical substance, I found that the elevations were occasioned by small protuberances on the surface of the alburnum. As the spring advanced, many minute red points appeared to perforate the bark: these soon assumed the character of buds, and produced shoots, in every respect similar to those which would have sprung from the organized buds of the preceding year. Whether the buds thus reproduced derived any portion of their component parts from the bark or not, I shall not venture to decide; but I am much disposed to believe that, like those of the potatoe, they sprang from the alburnous substance solely.

The space, however, in the annual root, between the medulla and the bark is very small; and therefore it may be contended that the buds in these instances may have originated from the medulla. I therefore thought it necessary to repeat similar experiments on the roots and trunks of old trees, and by these the buds were reproduced precisely in the same manner as the annual roots: and therefore, conceiving myself to have proved in a former Memoir,* that the substance which has

Other instances of fruit trees in which reproduced buds appeared to spring from the alburnum.

They do not originate from the medulla.

been called the medullary process does not originate from the medulla, I must conclude that reproduced buds do not spring from that substance.

Remarks on the manner in which this process of nature is probably effected.

I have remarked, in a paper which you did me the honour to lay before the Royal Society in the commencement of the present year, that the alburnous tubes at their termination upwards invariably join the central vessels, and that these vessels, which appear to derive their origin from the alburnous tubes, convey nutriment, and probably give existence to new buds and leaves. It is also evident, from the facility with which the rising sap is transferred from one side of a wounded tree to the other, that the alburnous tubes possess lateral as well as terminal orifices: and it does not appear improbable that the lateral as well as the terminal orifices of the alburnous tubes may possess the power to generate central vessels; which vessels evidently feed, if they do not give existence to, the reproduced buds and leaves. And therefore, as the preceding experiments appear to prove that the buds neither spring from the medulla nor the bark, I am much inclined to believe that they are generated by central vessels which spring from the lateral orifices of the alburnous tubes. The practicability of propagating some plants from their leaves may seem to stand in opposition to this hypothesis; but the central vessel is always a component part of the leaf, and from it the bud and young plant probably originate.

Attempt to discover the same power in seeds.

I expected to discover in seeds a similar power to regenerate their buds; for the cotyledons of these, though dissimilar in organization, execute the office of the alburnum, and contain a similar reservoir of nutriment, and at once supply the place of the alburnum and the leaf. But no experiments, which I have yet been able to make, have been decisive, owing to the difficulty of ascertaining the number of buds previously existing within the seed. Few, if any, seeds, I have reason to believe contain less than three buds, one only of which, except in cases of accident, germinates; and some seeds appear to contain a much greater number. The seed of the peach appears to be provided with ten or twelve leaves, each of which probably covers the rudiment of a bud, and the seeds, like the buds of the horse-chestnut, contain all the leaves and apparently all the buds of the succeeding year: and I have never been able to satisfy myself that all the buds were eradicated without having

having destroyed the base of the plumule, in which the power of reproducing buds probably resides, if such power exists.

Nature appears to have denied to annual and biennial plants (at least to those which have been the subjects of my experiments) the power which it has given to perennial plants to reproduce their buds; but nevertheless some biennials possess, under peculiar circumstances, a very singular resource, when all their buds have been destroyed. A turnip, bred between the English and Swedish variety, from which I had cut off the greater part of its fruit-stalks, and of which all the buds had been destroyed, remained some weeks in an apparently dormant state; after which the first seed in each pod germinated, and bursting the seed-vessel, seemed to execute the office of a bud and leaves to the parent plant, during the short remaining term of its existence, when its preternatural foliage perished with it. Whether this property be possessed by other biennial plants in common with the turnip, or not, I am not at present in possession of facts to decide, not having made precisely the same experiment on any other plant.

I will take this opportunity to correct an inference that I have drawn in a former paper,* which the facts (though quite correctly stated) do not, on subsequent repetition of the experiment, appear to justify. I have stated, that when a perpendicular shoot of the vine was inverted to a depending position, and a portion of its bark between two circular incisions round the stem removed, much more new wood was generated on the lower lip of the wound become uppermost by the inverted position of the branch, than on the opposite lip, which would not have happened had the branch continued to grow erect; and I have inferred that this effect was produced by sap which had descended by gravitation from the leaves above. But the branch was, as I have there stated, employed as a layer, and the matter which would have accumulated on the opposite lip of the wound had been employed in the formation of roots, a circumstance which at that time escaped my attention. The effects of gravitation on the motion of the descending sap, and consequent growth of plants, are, I am well satisfied, from a great variety of experiments, very great; but it will be very difficult to discover any method by which the extent

Annual and biennial plants have not this power.

Correction of a former inference.

* Phil. Trans. of 1803.

of its operation can be accurately ascertained. For the vessels which convey and impel * the true sap, or fluid from which the new wood appears to be generated, pass immediately from the leaf-stalk towards the root; and though the motion of this fluid may be impeded by gravitation, and it be even again returned into the leaf, no portion of it, unless it had been extravasated, could have descended to the part from which the bark was taken off in the experiment I have described. I am not sensible that in the different papers which I have had the honour to address to you, I have drawn any other inference which the facts, on repetition of the experiments, do not appear capable of supporting.

I am, &c.

THOS. ANDREW KNIGHT.

Elton, May 12, 1805.

XV.

*Experiments on the Gaseous Oxide of Azote, by a Society of Amateurs at Toulouse. Published by M. P. DUBAR, Professor of Chemistry in the College of that City.**

Disagreement of former experiments on the oxide of azote.

THE motive for the following experiments was the very different, and even contradictory results, which have been published of former effects. The experiments were tried upon more than a dozen persons, and in some cases repeated two or three times; the sensations which each experienced were written down at the moment, by the reporter, from whose memorandums the subsequent observations are drawn.

Preparation of the nitrate of ammonia.

The nitrate of ammonia used for the experiment was indistinctly crystallized, but was quite neutral. Its taste was very pungent, with a slight odour. It had been formed by the saturating very pure nitric acid with ammoniacal gas obtained by distilling sal ammoniac with the common potash of commerce.

Process for obtaining the gaseous oxide of

About one hectogramme (1545 grains) of this salt was put into a small retort, and placed on a sand-bath, where the salt

* Phil. Transf. of 1804.

† Annales de Chimie, Vol. LVI. p. 243.

melted and boiled for a short time without yielding any gas; at length, the retort became filled with a white vapour, which quickly disappeared; the gas was then rapidly disengaged, and was caught in bladders. By degrees the disengagement became more and more slow, and when the operation was ended, scarcely any thing remained in the retort.

Another experiment was made with a larger retort, and three hectogrammes (10 oz. troy) of the salt, from which was obtained gas sufficient to fill eight bladders. This operation proceeded in a similar manner with the former; except that as the retort cooled, a red vapour arose within it, which it was ascertained by experiment, contained no nitrous gas.

The same process on a larger scale.

Effects of Gaseous Oxide of Azote when breathed into the Lungs.

All who have tasted or inhaled this gas, agree in describing its flavour as strongly saccharine, and remaining upon the organs of some persons during the whole day after receiving it. M. Dispan observed in it an after-taste of nitre; but acknowledges that it was the last collected gas which he tasted.—M. de M***, perhaps under a similar impression, says he perceived in it a styptic quality.

The gas has a saccharine taste.

The method of respiring this gas was by means of a bladder with a stop-cock in it, applied to the mouth; the nostrils being closed, and the lungs as much as possible emptied.

The gas was inspired.

No. 1. The first person upon whom the experiment was tried, swooned at the third inspiration, and remained senseless about five minutes, when he recovered, but with a sensation of great fatigue. He recollected to have experienced only a sudden faintness, attended with a tingling at the temples.

No. 2. M. de M*** observed a saccharine and styptic taste, and experienced a sense of great dilatation, accompanied with heat in the breast; his veins swelled, and his pulse was quickened: surrounding objects seemed to revolve round him. But he thought he could have borne a stronger dose; the bladder not being large enough for his lungs.

No. 3 experienced a saccharine taste on the first inspiration; but became insensible to those which succeeded. His lungs were forcibly dilated with great heat. When the bladder was removed, he appeared very comfortable, but could not refrain from violent bursts of involuntary laughter.

No. 4 had the same saccharine taste with the preceding, and retained the impression from ten o'clock in the morning till after midnight. He experienced vertigoes, and his legs trembled under him during the remainder of the day.

No. 5, the same saccharine taste. On quitting the bladder, he had a dizziness of sight, which was succeeded by a sensation of great pleasure throughout the body. His legs were weakened.

No. 6. Saccharine flavour throughout the day; tingling in the ears; legs tottering, and the stomach oppressed. All that he experienced was rather painful than agreeable.

Receiving the gas from a bladder, had no influence on the result of the experiment.

Oxygen gas differed from common air only by a small increase of the heat of the lungs.

Conclusion.

Other experiments.

Description of the apparatus with upwards of albs. of the salt.

Particulars of the process.

In order to ascertain what influence the mode of breathing from a bladder might have on the foregoing results, the parties were requested to inspire common air in the same manner.—They were all mechanically fatigued by it, and nothing more.

The bladders were next filled with oxygen gas, and applied as before to the same persons, who found only a slight difference between it and common air, consisting in an augmentation of the heat of the lungs.

The singular effects above described, can, therefore, and ought only to be ascribed to the gaseous oxide of azote.

Another meeting of the society was held for repeating the experiments more at large, on the respiration of gaseous oxide of azote.

Eight hectogrammes ($27\frac{1}{2}$ oz. troy) of nitrate of ammonia, prepared as before, were put into a retort, with its neck fitted to a double-bodied receiver, from whence, by means of a tube of welter, the gas passed into an inverted vessel over water. The retort was placed on a sand-bath.

As soon as the heat affected the retort, the salt melted; and nearly at the same moment, sparkling vapours arose in the retort, but in very small quantity. The air which the heat expelled from the vessels had a nitrous odour; but this as well as the vapours gradually diminished, and as the process continued they disappeared altogether; they were succeeded by a lively smell of prussic acid. At length the retort became filled with white vapours, and the gaseous oxide of azote began to pass over. The disengagement soon became so abundant that it was judged proper to draw out the fire; but afterwards, on replacing the coals, the gas, which in the interval had diminished,

milled, was again so rapidly developed that the luting of the vessels began to give way. But notwithstanding the loss which this occasioned, the disengagement continued extremely rapid in the receiver for at least a quarter of an hour.

M. Dispan supposes, that if the luting had not given way, Danger of explosion would have taken place, as has happened to others in this process.

He next proceeds to state the effects of the respiration of this gas.

Twelve persons underwent the experiment, and on many The effects produced by the last gas were more powerful than the former. He observes that most of them had inhaled the gas of the former operation, where two out of seven experienced pleasing sensations; but on this second occasion, not one felt pleasure; on the contrary, they all felt pain, and many suffered extremely.

One person stamped with his foot the whole time of the breathing: when the bladder was removed, he recovered from the profound stupor into which he had been plunged, and complained of a pain in the back part of his head, as if he had received a violent blow from a dagger: he could not be prevailed on to make another trial. The other persons in general were affected with vertigoes and dizziness of sight, succeeded in some by involuntary convulsive fits of laughter.

M. Dispan tried the effects of this gas on himself, which he thus describes:—

"At the first inspiration, I emptied the bladder, and my mouth was instantaneously filled with a saccharine flavour, which extended into my lungs and inflated them. I emptied and filled them again; but on the third attempt, my ears were filled with a tingling noise, and I dropped the bladder. I did not, however, become altogether insensible, but remained in a kind of benumbed astonishment, rolling my eyes about without fixing them on any particular object: I was then suddenly seized with convulsive laughing fits, such as I never in my life before experienced. In a few seconds this propensity to laugh stopped suddenly, and I no longer felt any unpleasant symptom."

M. Dispan's Description of the effect of the gas upon himself.

Two others on whom the gas was tried, experienced only a convulsive movement of some of the muscles of the face; but were in the course of the day attacked with violent diarrhoea.

Effects on two other persons.

Difficulty of reducing the effects of this gas to any general system.

M. Dispan thinks it will be very difficult to reduce the effects of gaseous oxide of azote to any general system, as they vary so considerably in their operations upon different individuals, and, what is more singular, even upon the same person.

M. D. concludes his paper with an account of an experiment to ascertain the effect of gaseous oxide of azote upon animals.

Experiments on a bird immersed in gaseous oxide of azote.

He placed a greenfinch in a vessel of sufficient dimensions, and filled it with gaseous oxide of azote. At first, the bird seemed to suffer no inconvenience; but he soon gradually closed his eyes, and dropped gently on his side, as if asleep. On being restored to the pure air, he resumed his feet, without attempting to fly away. About an hour afterwards he was subjected to a second trial, and having been suffered to remain longer in the vessel, he was taken out quite dead.

M. Dispan thinks it very remarkable that the bird should make no effort to escape, and that he should manifest no convulsive symptoms, such as take place in experiments with other gases.

XVI.

*Observations on the Mammoth, or American Elephant, by which it is proved to have been an herbivorous Animal. In a Letter from the Right Reverend Bishop MADISON.**

Discovery of a mammoth having vegetable remains in its stomach.

ONE of those facts has lately occurred, which the naturalist knows best how to appreciate, and which I therefore take a pleasure in communicating to you. It is now no longer a question, whether the Mammoth was a herbivorous or carnivorous animal. Human industry has revealed a secret, which the bosom of the earth had, in vain, attempted to conceal.—In digging a well, near a Salt-Lick, in Wythe-county, Virginia, after penetrating about five feet and a half from the surface, the labourers struck upon the stomach of a mammoth. The contents were in a state of perfect preservation, consisting

* To Benjamin Smith Barton, M. D. editor of the Philadelphia Medical and Physical Journal, from which (vol. II.) it is taken.

of half masticated reeds, twigs, and grass, or leaves. There could be no deception; the substances were designated by obvious characters which could not be mistaken, and of which every one could judge; besides, the bones of the animal lay around, and added a silent, but sure confirmation. The whole rested upon a lime-stone rock. I have not seen, as yet, any part of those contents; for, though I was within two days' journey of the place where they were found, I was so well satisfied with the narration of gentlemen who had seen them, and upon whose veracity, as well as accuracy, I could rely, that I thought the journey unnecessary; especially as I took measures to ensure the transmission of a sufficient quantity of the contents, together with all the bones, to Williamsburgh. When the contents arrive, a part shall be forwarded to you. I hope to form a complete skeleton of this vast animal, having given directions to spare no labour, in digging up every bone.

We should not be surpris'd, that these substances should be thus preserved, when we recollect the state of the rhinoceros, mentioned by Pallas. Blumenbach, in his *Manuel d'Histoire Naturelle*, vol. II. p. 398, (traduit par Artaud), has a note, which is very applicable to the present subject. He says, "Quelquefois on trouve encore des piéces animales qui ont conservé, sans alteration, leurs parties molles; mais, cependant, comme elles se trouvent aussi enfouies dans la terre par la suite de ces grandes catastrophes des temps antérieurs, on doit les ranger parmi les corps pétrifiés, dans le sens le plus étendu. Je citerai, par exemple, le rhinocéros deterré près de Wiloi, en Sibérie, qui offroit encore des restes tres-reconnoissables, même ayant encore l'odeur animal de muscles, de chair, de peau, &c de poils. Pallas l'a décrit tres-exactement dans les Nov. Comment. Petropolit., tome 13. p. 585."

Remarks on the preservation of those bodies,

Whether this first kind of petrification, of which Blumenbach speaks, and which he calls *simplement calciné*, has been the cause of the preservation of these substances, or whether it be the effect of the marine salt, with which the earth, where they were buried, has been constantly charged, must be left to future investigation. I pretend not to decide. Had they been buried deep in the earth, that circumstance alone might have prevented a decomposition; but the depth of five or six feet seems insufficient to arrest that chemical action, which

—probably from marine salt.

changes the appearances of organized bodies. The fact, however, is decisive, as to the principal question. It has summoned the discordant opinions of philosophers before a tribunal, from which there is no appeal.

Williamsburgh, October 6th, 1805.

Note on the preceding Paper. By the Editor.

Facts by Mr.
Nevil on long
preserved vegetable
bodies.

Mr. Francis Nevil, in his account of the elephantine teeth that were discovered in the north of Ireland, early in the eighteenth century, has mentioned some facts relative to the long preservation of vegetable matters, which seem worthy of our notice in this place: and the more so, as this gentleman's paper seems not to have excited any attention among the modern writers on the exuvæ of animals found in countries in which the living animals themselves are no longer seen. Some extravagant conjectures are mixed with Mr. Nevil's account: but these do not, in the least, invalidate the truth of what he says, relative to the bed upon which the Irish elephant was laid.

His narrative.

"The place (says he) where this monster lay, was thus prepared, which makes me believe it had been buried, or that it had lain there since the deluge. It was about four feet under ground, with a little rising above the superficies of the earth, which was a plain under the foot of a hill, and about thirty yards from the brook * or thereabout. The bed whereon it lay had been laid with fern, with that sort of rushes here called sprits, and with bushes intermixed. Under this was a stiff blue clay on which the teeth and bones were found: above this was first a mixture of yellow clay and sand much of the same colour; under that a fine white sandy clay, which was next to the bed: the bed was for the most part a foot thick, and in some places thicker, with a moisture clear through; it lay sad and close, and cut much like turf, and would divide into flakes, thicker or thinner as you would; and in every layer the seed of the rushes was as fresh as if new pulled, so that it was in the height of seed-time that those bones were laid there. The branches of the fern, in every

* "A small brook that parts the counties of Cavan and Monaghan."

My as we opened them, were very distinguishable, as were the seeds of the rushes and the tops of the boughs. The whole matter felt very soft as it was dug, and tracing it I found it 34 feet long and about 20 or 22 feet broad."—"I forgot to mention that there was a great many nut-shells found about the bed, perhaps those might have been on the bushes which composed part of the bed *."

• XVII.

Observations on the Danger of using Earthen-ware or Pottery of a bad Quality. By M. GIDEVIN of Rouen†.

PURE white argil forms the body of the finest pottery which bears the name of porcelain; clays less pure, and coloured more or less with iron, serve to form the stone ware, or hard earthen-ware, and the common or soft sort, which differs from the other, in not experiencing a commencement of fusion at their surface in baking, like porcelain or stone ware.

Different kinds of pottery.

This badly prepared common earthen-ware is the kind which is occasionally attended with danger in its use, and is the subject of this paper.

Earthen-ware.

The biscuit of brown earthen-ware is prepared from a ferruginous clay; that of white earthen-ware is composed of a mixture of ferruginous clay, of another clay containing much silicious sand, a little lime, and finally of a porous clay, which renders it less compact, and gives it whiteness after baking.

Common brown ware.

Nature not always affording these earths in the same state of combination, occasions differences in the biscuit, when it becomes subjected to the heat: other differences also arise in the action of the enamel on the biscuit. If the earth be too ferruginous, or too much mixed with silicious particles, the enamel, during the baking, acts as a flux on the biscuit, softens it, and occasions the pieces to lose their shape.

Differences in the ware from the quality of the materia

If the earth is too porous it absorbs the enamel and remains

* A Natural History of Ireland, in three parts, by Dr. Gerrard Boate, Thomas Molineux, M.D. F. R. S. and others. Pages 128—130. Dublin: 1755.

† Annales de Chemie, T. 55.

rough,

rough, and as it were dried. If it contains too much lime, it throws off the enamel, which falls from it in scales instead of adhering to it.

Composition of the enamel or glaze.

On the other hand, the white enamel is composed of silicious sand, a little lime, lead and tin oxides, and some flux, ground together with water in mills. The brown sort is composed of the same materials, with the addition of manganese and perigord stone*.

Causes which occasion variations in the glaze.

The greater or less fusibility of the sand; the greater or less purity of the lead, of the tin, and of the saline substances employed as fluxes; the different degrees of heat which the mixture receives in the glazing; the variations of the fineness given to the glazing materials by the action of the mill, are so many circumstances which cause changes in the enamel in its state of fusion on the pieces, relative to the state in which it finds the biscuit and to the fusible layer, with which this last is covered.

Pottery.

Brown pottery.

The body of the brown pottery is a red clay, more or less ferruginous and compact according to the places where it is procured.

Yellow pottery.

The common or yellow pottery is made of a white clay, which contains a little lime and magnesia, and a considerable quantity of silicious sand, which may be generally esteemed a fourth of the mass.

Glaze for brown ware.

The glazing of the brown pottery is formed with a mixture of silicious sand, yellow or red oxide of lead, and manganese pulverised together.

Glaze for yellow ware.

That of the yellow earthenware is composed of a mixture of silicious sand, and red oxide of lead, which, during its baking vitrifies at its surface, and forms a yellow glazing more or less transparent. To this mixture is commonly added, in France, a little oxide of manganese in powder, more or less fine, without grinding them together. This is called the *grain*, because it fuses more difficultly than the other materials, without mixing with them, and by that means forms streaks, spots, or brown specks, according to the coarseness of the powder itself.

Cloudy rings in the glaze.

In some manufactories they mix oxide of copper with the common glazing, to give it a green colour, and in others they form designs on the pieces, with oxide of copper, which pro-

* A black stone or compact manganese. T.

...uces agree, with oxide of iron, which causes a red, or with oxide of manganese, which gives a brown.

Great imperfections are produced in pottery, from the injudicious use of glazing over earths of an unsuitable nature, and this is more remarkable when the earths are not so well prepared for their glazings as they are for those of the finer wares. The articles of common pottery are less carefully prepared both in their materials and baking. This last is usually performed at a single operation, and with less fire.

Causes of imperfection in pottery.

The means of producing good pottery and earthen-ware, consist in carefully chusing the earths for forming the body : in producing an exact coincidence of expansion by heat between them, and the vitrifiable glaze with which they are to be covered, and in baking them by a proper degree of fire, produced from combustibles not capable of changing the nature of the glazing.

Cautions requisite to insure its goodness.

The neglect of these attentions occasion defects in the manufactured articles, which are either unsightly and nothing more, or both unsightly and dangerous.

The unsightly defects which are found in ill conditioned pottery or earthen-ware, are, *scaling*; the *dropping* or *drops*; *smoke*; *drying* of the ware, and *flaws* or *cracks*.

Defects or deformities enumerated.

The *scaling* is the appellation used when the glazing of a piece detaches itself in scales, by the action of moist air, or on the least touch, and leaves the biscuit uncovered.

Scaling of the glaze.

The *dropping* or *drops* take place when the moisture of the fuel having struck the pieces during the baking, the enamel is collected in *drops* on the surface, and remains vitrified in that form, instead of being equally spread.

Dropping or drops.

The smoky appearance happens when a piece has not been purified by a clear flame, but remains blackened or stained.

Smoky tinge.

The *drying* happens when the pieces are, as it were, roasted in the firing, and come out rough from the absorption of the enamel into their substance.

Drying.

The *flaws* happen, when the earth or the biscuit, having a different pyrometrical expansibility from that of the enamel, the body contracts in cooling more than the glaze which is therefore split, or which is divided into an infinite number of small parts, sometimes not perceptible to the eye when the pieces are new, but which become very visible, when the goods have imbibed any greasy substance in using.

Flaws.

All these defects, though disagreeable to the eye, have really, The coarse cry is most defective.

The coarse cry is most defective.

really, with regard to the ware itself, only the inconvenience of a dirty appearance, provided the biscuit is always compact, and well baked. But it is different in the common pottery in which the dropping, the scaling, and the flaws produce more injurious defects. As the earth is more porous and less baked in those, the liquids preserved in them enter into the pores where they become altered and decomposed, and produce sulphurated hydrogen, which injures every thing kept in them.

Cavities or pits
from bad firing.

The most noxious defects in pottery are the cavities or pits, and the underbaking. The pits are roughnesses or hollow bubbles which are found on those pieces, whose enamel being injured by rubbing, or being too little acted on by the fire, has not been fused into a vitreous substance. In these the metallic oxides are in a state capable of doing injury, being still soluble in fat or acid substances.

Underbaking or
imperfect fusion
of the

I.e. underbaking occasions one of the most dangerous defects in pottery; the pieces thus affected have not had sufficient heat to cause the enamel to do more than agglutinate together, and in some cases it even still remains in powder. It is therefore capable of being divided, and taken up by all the liquids with which it may come in contact.

It is easy to shew the danger to which the public must be exposed in buying those articles at a low price which are called waste or refuse and which ought to be carefully thrown away. In vain may it be said that they are used daily without any immediate mischief happening; from the injury being more concealed, it is no less destructive. It is known that lead and its oxides act insensibly on the organs of digestion, especially when taken in small quantities: They do not, however, less certainly cause, at length, emaciation, cholics, convulsions, sometimes of all parts of the body, with obstinate diarrhoeas; and the wretched people who use such vessels become the victims of their own ignorance, and of the imprudent advice of the manufacturer.

It would be to the honour of enlightened manufacturers, not to offer to the public pieces which have imperfections beyond a certain degree, and to make this sacrifice to the good of national commerce, especially as they can avoid the loss by a greater attention to their materials.

XVIII.

Extract of a Letter from M. JOHN MICHAEL HAUSSMANN,
to M. BERTHOLLET, on the Existence of intermediate Terms
of Oxidation. * *

I THINK there are sufficient grounds for admitting, with you, that there exist, in the oxidation of many metallic bodies, intermediate degrees between the *minimum* and the *maximum*. Existence of intermediate degrees of oxidation of metals.

The first example I shall cite, is, that of a *minimum* oxide of tin, precipitated from the muriatic solution, and dissolved in an excess of caustic potash; metallic alkaline solution which I have before noticed in my Observations on the Red Dye of Adrianople, inserted in the "*Annales de Chimie*," and also in a Memoir on the coloured Oxides of Tin, inserted in the "*Journal de Physique*." Oxide of tin.

By avoiding any dilution of the muriate of tin, and using a very concentrated solution of caustic potash, the mixture disengages much caloric, part of the tin is precipitated in the metallic state, whilst the remainder is held in solution in an intermediate state of oxidation. This alkaline solution is so disoxidant, that it changes the yellow oxide of gold, fixed on cotton, by means of ammonia, to a grey; whilst a similar yellow pattern underwent no change of colour on being steeped in the simple liquor of caustic potash. A like alteration took place on dipping a cotton cloth, which had been previously stained with the solution of gold, and well dried in the alkaline solution of tin, which also produced the same effect on pouring into it the pure solution of gold diluted with water. Experiment. Muriate of tin is in part precipitated metallic and the rest suspended with intermediate oxidation.

This change of the yellow colour of oxide of gold by the alkaline solution of tin, is not the only proof of an intermediate state of oxidation; this liquor possesses besides, a property of destroying the blackish-brown colour of the oxide of manganese stained upon cotton by an alkaline precipitant. Other proofs of that intermediate state.

All these changes are more rapidly produced, if, prior to the precipitation and solution in the caustic potash liquor, the muriatic solution of tin be diluted with six or eight parts of water, in which case there is no sensible disengagement of

caloric, and no tin is precipitated in the metallic state. This solution, whose oxidation approaches the degree of *minimum*, for the most part retains an aqueous transparency, without any precipitation of oxide; even when long exposed to the atmospheric air, it does not lose the property of changing the yellow oxide of gold to a grey colour, or of destroying the blackish brown tint of oxide of manganese, when fixed upon cotton.

Oxide of manganese.

The oxide of manganese is capable of various degrees of oxidation; if a piece of cotton cloth be dipped in the transparent solution of sulphate of manganese, it will, when dry, retain its original whiteness; but on their dipping the same cloth in the liquor of carbonated or caustic potash, it will, after washing and exposure to the atmospheric air, be coloured brown; which colour will acquire a deeper shade, approximating to black, on being steeped for a time in an oxygenated alkaline, muriatic liquor. The oxygenated alkaline liquor, on being for any length of time submitted to the action of the brown precipitate of manganese, instead of the rag steeped therein (which is to dissolve by means of an increased oxidation) will assume a purple colour, of greater or less transparency as the time of their union has been longer or shorter.

Other oxides.

There seems reason, generally, to expect particular results from submitting any of the metallic oxides to the action of this oxygenated muriatic alkaline liquor; which might, perhaps, be a means of giving them acid properties, and at the same time of proving the gradual oxidation of many metals; this is the more observable in white oxide of lead, which becomes gradually coloured by long exposure to the oxygenated liquor, and being frequently stirred.

Muriatic and nitro-muriatic solutions of tin, though colourless in themselves, acquire by admixture a vinous tint.

Muriatic and nitro-muriatic solutions of tin, well diluted with water, have an aqueous transparency, when properly made; but if the two be mixed together, they acquire a fine vinous colour, similar to that of Malaga; this can only arise from the oxygen of the nitro-muriatic being in part communicated to the muriatic solution of tin.

The addition of solution of gold produces a purple dye.

If a solution of gold with great excess of acid, and diluted with from 130 to 160 parts of water, be gradually poured into the above mixture, stirring it all the time, the intensity of the colour will be increased, till at length the liquor becomes

comes of a beautiful purple but in which all kinds of goods may be dyed; this may be changed to the tint of peach or lilac blossoms, by increasing the proportions of the nitro muriatic solution; or, on the other hand, by causing the muriatic solution of tin to preponderate; shades of grey will be obtained, deeper or lighter in colour, according to the quantity of the solution added. Care must, however, be taken, in the latter experiment, that too great a proportion of the muriatic liquor of tin be not used; for by depriving the oxide of gold of too much of its oxygen, it might be too much disoxidized and precipitated. The precipitate caused by such an accident is not altogether void of oxygen, which prevents its gilding cold silver, as do the ashes of burned cloth impregnated with the solution of gold. The degree to which the preservation of the tincture of gold may be carried, must depend on the proportions of the two solutions of tin, their being more or less surcharged with acids, and the quality of the solution of gold, wherein also there should be a very great excess of acid.

The precipitate of the oxide of gold will not gild silver, without the assistance of heat.

The purple tincture of gold, though of the most perfect transparency, is decomposed by exposure to a strong heat, and throws down what is known by the name of "*Purple of Cassius*," whose beauty depends on the quantity of nitro-muriatic solution of tin made use of. The latter, however, if mixed alone with the solution of gold, without the presence of muriatic of tin, produces no alteration of colour, and, if the mixture be not too much weakened with water, is a very long time before it gives a precipitate.

Purple powder of Cassius.

The purple tincture of gold, is, properly speaking, nothing more than the powder of cassius, held in solution by means of the oxygen of the nitro-muriatic liquor of tin; and there is every reason to believe, that in the powder of cassius, the oxide of gold is in some way combined with the oxide of tin, which, by transmitting to it its own origin, during its fixation upon porcelain, prevents it, I think, from returning to its metallic state. I find a difficulty in subscribing to the opinion of Dr. Richter, of Berlin, who, in a memoir (which I have not read) attempts to prove, by mathematical demonstration, that the crimson-coloured gold on porcelain is in the metallic state.

Dr. Richter's opinion of the crimson-coloured gold upon porcelain.

The purple tincture of gold might be advantageously employed in dyeing silks, without greatly enhancing the price.

Purple tincture of gold surpasses all others in dyeing silks.

The dyeing silks.

The colour obtained from it surpasses all others in duration, since nothing less than combustion can destroy it. It is necessary, however, to leave the silk a long time in the dye; and the depth of the shade will be in proportion to the number of times the article is dipped; it must be well wrung, rinsed, and dried, between each immersion.

The gradation of shades already noticed are indications of a gradual oxidation.

Sulphate of iron loses its excess of oxygen by ex-

The gradation of colours produced by mixture of the nitromuriatic, and muriatic solutions of tin, being much weakened by dropping solution of gold in a great excess of acid, considerably diluted with water, into the mixture, seems to me to indicate a gradual oxidation. The acetic solution of iron proves the same truth; for on being exposed to the atmospheric air, or to the contact of oxygen gas, it gradually changes from a sea green to a reddish yellow colour. I have shown, in a memoir on the alkaline Tincture of Mars of Stahl, that sulphate of iron may be super-oxygenated, and also lose its excess of oxygen by the action of light. On mixing concentrated sulphuric acid with nitric solution of iron, I obtained, after the nitric acid was evaporated, by leaving the residuum to imbibe the moisture of the air for several months; crystals of super-oxygenated sulphate of iron, which were at first distinguishable by their whiteness from sulphate of alumine; but the action of the light gradually tinged their surface with a yellow colour; their original whiteness, however, might, by a gentle washing, be restored at pleasure. Super-oxygenated sulphate of iron, of nearly an equal degree of whiteness, may in like manner, be obtained by precipitating nitrate of iron, and dissolving the precipitate,edulcorated and freed from water, gradually in sulphuric acid, which, if well concentrated, will produce crystals of super-oxygenated sulphate of iron without evaporation. This salt possesses an incomparable degree of astringency.

The fact that linens printed with acetate of iron are liable to become rotten, a proof of the gradual transmission of oxygen.

The progress of the transmission of oxygen is more manifest on linen simply printed with acetate of iron and madder, which must be a long time exposed in the air to bleach, unless the artificial means of bleaching be adopted. The printed part of the linen frequently perishes, bearing the appearance of having been cut with a sharp instrument, or burned with concentrated acid; this, it should seem, must proceed from the action of the oxygen contained in the coloured oxide of iron, continually replenished from the atmospheric air.

It

It is not among minerals alone that substances are found which are gradually oxidized, and by intermediate degrees.

Indigo affords an instance that vegetable and animal bodies offer similar proofs; for any solution of indigo (excepting the sulphate of indigo) will, on disoxidation, or on having its oxygen restored, pass through all the degrees of shade, from blueish green to very yellow olive, preserving in the mean time the same quantity of indigo in solution. The beauty and stability of the colours, either for dying or painting, will chiefly depend on the degree of oxidation. On some other occasion, Sir, I shall write to you more amply on this subject.

Vegetable and animal bodies furnish similar cases with minerals, of gradual oxidation.

SCIENTIFIC NEWS.

Mémoires de l'Académie impériale des Sciences, &c. Memoirs of the imperial Academy of Sciences, Literature, and fine Arts, of Turin, for the Years 12 and 13, 2 Vols. Quarto. 1805. Turin.

WHEN the Royal Academy of Turin assumed the name of Imperial, in consequence of Piedmont being annexed to France, the number of academicians was increased, to form a new class, that of literature and the fine arts. Of the two volumes published, one is appropriated to the labours of this class, the other to that of the physical and mathematical sciences.

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

The latter is compiled by the secretary, Mr. Vassali Eandi, who first mentions the changes that have taken place in the list of academicians, next the various papers that have been read at their meetings, and then the books and other articles presented to the society. These lists are followed by a well written account of the labours of the academy up to the year 1805, which occupies 250 pages. After this follow the different memoirs.

1. Description and use of a new portable barometer, for measuring heights and depths, with observations made with this instrument in the circles of Turin and Saluzzo. This instrument, of which a figure is given, was invented by the secretary; who has subjoined to his paper some very curious historical notes on the places where his observations were made.

Memoirs of the
Imperial Academy of Sciences,
&c. of
Turin.

2. Account of a waterspout, that occurred in the territory of Revel, in the circle of Saluzzo, March 27, 1798, with remarks on the cause of the phenomenon, by the same.

3. On the different capacities for conducting heat ascertained by experiment in different articles, used for clothing, by J. Sennebier.

4. Of a new species of hawkweed, *crepis*, to which are added some cryptogamiae of Piedmont, by J. Baptiste Balbis.

A figure of this plant, which Mr. B. calls *crepis ambiguus*, is given. Among the cryptogamiae are the following new species, *mucor flosculatus*, *peziza amentacea*, *lichen nivalis*. These likewise are figured.

5. Experiments on the effects of the nitric and oxygenated muriatic acid, employed topically in the treatment of various diseases, by Mr. Rossi. Mr. R. gives an account of the cure of several gangrenous ulcers, venereal buboes, and even contagious carbuncles, cured by the application of these acids.

6. Meteorological observations made during the solar eclipse on the 30th of Jan. 1805, at the observatory of Turin, with reflections on them, by Ant. Mar. Vassalli Eandi.

7. On a species of cassia, that may be substituted for the fenna of the shops, by Mr. Bellardi. This is the *cassia marilandica*, which Mr. B. would call *succidanea*, because, according to him, it may supply the place of the *cassia lancolata*.

8. Inquiries into the nature of the galvanic fluid, by A. M. Vassalli Eandi.

9. On the mines of plumbago in the departments of the Sture and the Po, by Mr. Ronvoisin.

10. Attempts to improve nut oil, by the same. Mr. B. points out a method of purifying this oil, and rendering it as fit for lamps as other fine oils.

11. Examination of the action of the galvanic fluid on different gases, by J. A. Giobert.

12. An anatomical and physiological essay on the lymphatic glands, by professor Rossi.

13. Solution of a problem depending on the theory of permutations and combinations, by professor Balbo.

14. Explanation of the circumstance of a fish being occasionally found with prickles in the river of the 27th military division.

division, by Mr. Giorna. The fish is the *cyprinus idus*; the male only has prickles, and loses them after spawning time. Memoirs of the Imperial Academy of Sciences, &c. of Turin.

15. A chemico-medical essay on the pulmonary consumption, by Jos. Hyaz. Rizzetti. The principal subject of this essay is the nature of the matter expectorated.

The following papers are by foreign members.

1. Memoir on the use of varying the constant quantity in summing up equations with variable coefficients, by Dr. Brunacci.

2. A systematical enumeration of the coleoptera found in the territory of Saluzzo, with observations, by Law. Ponza. To this catalogue are annexed two plates, containing the following new species. *Coccinella numeralis*,—*c. obsoleta*,—*curculio spinosus*,—*c. dubius*,—*c. rugosus*,—*cerambyx præustus*,—*c. melanocephalus*,—*chrysomela melanocephala*,—*ch. variegata*,—*ch. pretiosus*,—*ch. lucuosus*,—*scarabæus rufescens*,—*cantharis impressifrons*,—*attelabus funereus*,—*dytiscus filphoides*,—*tenebrio rufus*,—*birrhus rossii*,—*carabus attenuatus*,—*c. metallicus*,—*c. rossii*,—*forficula bipunctata*,—*filpha sinuata*,—*f. scabra*.

3. On the motion of the hairs of the hypnum *adiantoides*, by Palamedas de Suffren. Parts endowed with irritability had already been observed in the hairs of some mosses. Mr. De S. has found it in those of the *h. a.* and describes all the singularities of the phenomenon. This paper is accompanied with a plate.

4. Of a resin employed by the bee in constructing its combs. By Fr. Mouxy Deloche.

5. Entomological observations; by Mr. Disderi. Mr. D. first sketches the history of the silkworm; and then proceeds to certain hymenoptera, chiefly of the genera *tenthredo*, *ichneumon*, *sphex*, et *vespa*.

6. Specimen of the fungi of the vale of Pisa, by Hugh Camino. The new species are figured on three plates. They are *Agaricus elatior*; *a. miniatus*; *a. pezizoides*; *a. astrosanguineus*; *a. tricolor*; *Boletus scobinaceus*; *Helvella grandis*; *h. reflexa*; *h. inflata*; *Peziza aschacea*; *p. pyriformis*; *Reticularia rosea*; *Mucor fruticulosus*.

7. Observations on the native gold found among sand, by Lew. Bossi, of Milan.

Barometer.

MY correspondent from Edinburgh is reminded, with regard to his project for a barometer, that no enlargement or diminution of the bore will make the least difference in the scale of the common barometer, consisting of a tube or vessel, closed above, and having its lower end open, and communicating with a basin of mercury of considerable diameter.

Subdivision of an arc by wheel and chain.

The contrivance, received some time ago from T. I. for making an astronomical instrument, in which the angular quantities shall be measured by the communication of a chain, strap, or string, possesses so much ingenuity and promise, that it has exercised the heads and hands of a number of eminent men. Among these are Robert Hooke, for a quadrant; Muschenbroeck, for a pyrometer, and many operative men, such as Sisson and others, for theodolites and quadrants.— Where the intention of the instrument is simply to magnify the motion, without any particular attention to precision, the contrivance has a happy effect; particularly in public lectures, where a number of spectators may observe the same effect at the same time. It is likewise cheap, and may be carried into effect in situations where the use and application of more accurate apparatus cannot be referred to.

It cannot be made very exact.

A slight attention to the subject, will shew that all contrivances of the kind here alluded to must be considerably inaccurate. For they demand, 1, that the wheels should be very truly circular: 2, and free from all dirt and impurity: 3. that they be well centered: 4. that the chain or string should be every where of the same thickness: 5, and its tension in all positions alike, &c. &c. If the quantity of error, taken at a minimum, which must arise from these and other causes, be attended to, it will be found that a simple division of an arc (subdivided by a screw or a nonius) and examined or read off by a small magnifier, will afford greater precision; even when the work is performed by a careful designer, who is no mathematical instrument maker. It is certain that much greater delicacy and precision may be had in the division of mathematical instruments by the patient diligence of a cultivator of practical mechanics than is generally supposed.

JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO VOL. XIII.

ARTICLE I.

*On the Saline Efflorescences upon Walls; Salicary Concretions;
Deflagration of Mercury by Galvanism; Biliary Calculi; and
the freezing Point of Spermaceti. By JOHN BOSTOCK, M. D.*

To Mr. NICHOLSON.

SIR,

IN the third and sixth volumes of your Journal you have inserted an account of some experiments that I performed on the saline efflorescences found upon walls. I have lately had an opportunity of examining two other specimens, of which I now send you the particulars. The first was obtained in considerable quantity from the inner walls of a warehouse that had been erected about twenty years. By a series of simple experiments, which it is unnecessary to detail at full length, I found it to be a sulphate of soda, which, as in the former cases, seemed to exist in a state of almost perfect purity. The circumstances attending the second of these efflorescences were more singular. It was given me by a friend who had scraped it from off the stones which are situated on the inside of the west side of York Minster. My friend, on whose

Examination of two specimens of efflorescence found upon walls.
The first was sulphate of soda
The second was scraped from the

Dd curacy

face of the stone
within the York
Minster, and not
from the mortar.

It was sulphate
of magnesia,
very pure.

curacy I place the fullest confidence, expressly stated, that it was taken from the surface of the stone itself, and not from the joints, or any part that had been covered with mortar. It existed there in large quantity, and was disposed in the form of projecting spiculæ. Upon subjecting it to the usual trials, I found it to be a very pure sulphate of magnesia. In order to ascertain with precision the degree of its purity, I prepared a quantity of the sulphate of magnesia, by uniting together its constituent parts. This artificial salt, and the salt from York, after being crystallized, were exposed for some time to the same degree of heat, and when all the water of crystallization appeared to be expelled, equal weights of them were dissolved in equal weights of water: 100 grains of these solutions had the muriate of barytes respectively added, until no farther precipitation was produced, when it appeared that exactly the same weight of barytes was necessary to saturate each solution. The portions of precipitated sulphate of barytes were collected and dried, and when examined by a nice balance, exhibited scarcely any perceptible difference in weight; they each amounted to 7.9 grains. A similar process being adopted with respect to the common Epsom salt of the shops, the precipitate was found to be 7.35 grains only. Before I quit this subject, I may remark that another friend, in visiting the cathedral at Tewkesbury, noticed a saline efflorescence on the inside of some part of that building; he collected a portion of it, intending to give it me for examination; but it was accidentally lost. Perhaps some of your readers, who reside in that neighbourhood, may be induced to examine it, and transmit the result to your Journal. I confess myself totally unable to explain the production of the sulphate of magnesia on the surface of a freestone, such as, I believe, forms the body of York Minster.

Qs. Whence
came the mag-
nesia?

Account of a
concretion in the
salivary ducts.

Among the solid concretions which are formed in different parts of the human body, those from the salivary ducts are occasionally met with. I lately procured one of these substances, of which I will give you a brief account. It was a cylinder, pointed at one end, of half an inch in length, and somewhat more than $\frac{1}{2}$ of an inch in diameter; it weighed $1\frac{1}{2}$ gr. It was white and smooth on the outside, and its internal fracture did not exhibit any marks of regular organization. To half a grain of the concretion a few drops of diluted

muriatic.

muriatic acid were added; no effervescence was excited. By the application of a gentle heat the whole was dissolved, except a few films that swam in the fluid. A copious precipitation was produced in this solution by pure ammonia, but none by the carbonate of ammonia. A part of the muriatic solution was evaporated; the residue was not soluble in water, but was speedily re-dissolved by the muriatic acid. The muriatic solution, saturated with the carbonate of ammoniac, had a precipitation produced by the oxalate of ammoniac. It appears therefore that the concretion consisted of the phosphate of lime, mixed with a little animal matter, probably coagulated albumen; it did not contain any carbonate of lime, and its component parts appeared not to possess any regularly organized structure. M. Fourcroy* and Dr. Thomson† have examined similar bodies, and agree in considering the earthy matter to be the phosphate of lime; we may therefore reasonably conclude that this substance always composes the earthy part of the salivary concretions. I am disposed, however, to differ from these distinguished chemists in my idea respecting the nature of the animal matter which enters into their composition; M. Fourcroy considers it as consisting of a species of mucilage, while Dr. Thomson describes it as "a membranous substance, which retains the shape of the concretion after the solution of the phosphate." This was certainly not the case with the one which I examined. I am disposed to consider the animal matter as coagulated albumen, rather than mucus, in consequence of its insoluble nature, and the greater facility with which it would on this account be detained by the phosphate of lime.

It was phosphate of lime chiefly,

—with coagulated albumen.

The power which the electric fluid possesses, when generated by the galvanic apparatus, of burning metallic plates, affords one of the most beautiful experiments of which the science of chemistry can boast. All the metals have by this means been subjected to combustion, except mercury, which, owing to its fluidity, is incapable of being formed into thin laminae. I have, however, been fortunate enough to accomplish this object, and that by the most simple method.

Mercury has not heretofore been deflagrated by galvanism.

* *Système*, IX. 363.

† *Chemistry*, IV. 658.

‡ *Thomson's Chemistry*, I. 125.

Experiment in which this was effected.

I was performing some experiments with Mr. Richard Dalton, an ingenious lecturer in natural philosophy of this place, with a pile composed of 60 pair of six-inch plates of zinc and copper, when it occurred to me to place a minute globule of mercury in an iron spoon, resting on the top of the pile, and to approach to it a thick iron wire connected with the other end of the apparatus; the effect was, that a brilliant star of light was produced from the mercury, attended with a crackling noise and a copious emission of sparks; the mercury was found converted into the black oxide.

The dark coloured particles of gall-stones do not appear to be inspissated resin of the bile.

The most common species of biliary calculus is that composed of the peculiar crystalline matter, which in some of its properties resembles spermaceti, through which are interspersed a number of dark coloured particles, that are supposed to consist of hardened bile. This is the idea entertained by M. Courcroy,* and the one which I adopted, when I made the experiments on this subject which are related in the fourth volume of your Journal. I have, however, since that time been disposed to alter my opinion; in two specimens of the biliary calculi, which I examined, after separating the crystalline matter by alcohol, I was unable to dissolve the dark coloured particles by any menstruum which I applied to them; they imparted a yellowish tinge to water and other fluids, but the great bulk of their substance remained unchanged. It is, I conceive, not probable that the mere inspissation of the resin of the bile could so far alter its properties. I mention this circumstance principally with a view of attracting the attention of any of your readers who may be in possession of a number of gall-stones, so as to ascertain whether the untractable nature of these particles is a general property of the cystic-adipobilious concretions, or something peculiar to the specimens upon which I experimented.

Melting point of spermaceti. Repetition of experiment confirms that it is a little above 112°.

I shall conclude this miscellaneous letter with some remarks upon the melting point of spermaceti. In the paper to which I have already referred, I mentioned the diversity of opinion that had been entertained on this subject, and afterwards stated that my own experience induced me to fix it at the 112th degree. Dr. Thomson, in the first volume of his Chemistry, fixes the melting point at 133°,† while in the fourth he states

it to be 112° , upon the authority of my paper.* Yet in his answer to the Edinburgh reviewers, he has mentioned this estimate of the melting point of spermaceti as one of his acknowledged errors, and upon the authority of Dr. Gibbes, fixes it at 115° . This circumstance determined me to repeat the experiment; I employed a very delicate thermometer, and used every requisite precaution; the result was that the instrument descended to *a little above the 112^{th} degree*, and remained stationary until the substance was become solid. I may add that Dr. Irvine, in some experiments related in the ninth vol. of your Journal, fixes the point at 113° , which agrees so nearly with my observations, as to afford me an additional confidence in their accuracy.

I am, Sir,

Your obedient servant,

JOHN BOSTOCK.

Liverpool, April 9, 1806.

II.

Investigation of the Temperature at which Water is of greatest Density, from the Experiments of Dr. Hope on the Contraction of Water by Heat at low Temperatures. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

IN your Journal for February 1805 was inserted a letter of mine containing certain facts relative to the subject of my present communication, which led me to disbelieve the common opinion that water is densest at 40° , and inclined me to think it is at 32° . Since that time my attention has again been turned to the subject; some small but immaterial corrections of the facts have been made and additional ones obtained, by which I have been enabled to demonstrate, at least to my own satisfaction, that the temperature at which water is of greatest density is at or near 36° of Fahrenheit. The results have lately been communicated to the Manchester Society, and may

Reference to a former communication, in which the maximum density of water was taken at 32° .
Present inference that it is a 36° .

perhaps appear in a future volume. My present object is to shew that the results of Dr. Hope's experiments are explicable on the supposition of water being densest at 36° , but on no other.

Observations on the expansions of water on each side of its point of greatest density.

Dr. Hope and myself concur in the opinion that water is densest at some one point of temperature, and that above and below that point it expands alike by heat and cold in a gradually increasing manner. De Luc was the first to observe that the expansion is the same quantity for the same number of degrees, whether of increase or diminution of temperature; the remarkable fact was extended by my former experience from a range of 8° to 25° or more, above and below the stationary point. I have lately examined this fact with greater attention to precision than formerly, and find that it is accurate, except that the expansion for degrees below the stationary point is always somewhat more than for a corresponding number of degrees above the said point. Thus, water is stationary in a glass thermometer at 42° ; if heated to 75° by the mercurial scale, it expands very considerably; if plunged into a frigorific mixture of 13° , it falls to 42° , and then expands again to the same point of 75° , at which it remains stationary as long as continued in the mixture. It may be remarked too, that congelation rarely if ever takes place in the bulb, when the mixture is not below 15° , which may easily be procured by putting snow into water saturated with common salt. Hence we see that 29° below, afford the same expansion as 33° above the stationary point. This, I imagine, is occasioned by the error attached to the equal division of the mercurial scale. For a small number of degrees, however, we may admit that the expansions for corresponding intervals above and below are equal; hence we obtain the following table of corresponding temperatures at which water is of the same density.

Supposing greatest density at 40°			at 36°		
39° and 41°			35° and 37°		
Corresponding densities will be at	38	42	Corresponding densities will be at	34	38
	37	43		33	39
	36	44		32	40
	35	45		31	41
	34	46		30	42
	33	47		29	
	32	48		28	44

Pr.

Dr. Hope also admits with ^{the} fact that water subjected to be cooled without agitation in a frigorific mixture, usually descends several degrees below the freezing point, and still retains its liquidity. Though it is easy to obtain water in a glass bulb 20 or 25° below freezing, I could never cool water in an open jar more than 10 or 11° below freezing, agreeable to the experience of Sir Charles Blagden. But I find water in such circumstances will admit of being cooled to 25°, and the bulb of a thermometer to be immersed and withdrawn several times, without freezing.

The author and Dr. Hope concur that water may continue fluid below its freezing point.

We come now to the experiments of Dr. Hope.

Experiment I.

A jar eight inches deep and $4\frac{1}{2}$ in diameter, filled with Dr. Hope's ex-water of 32°, and placed on a table, &c. Air 60—62°. Two thermometers inserted, one at the top, another at the bottom.

Top Thermometer.		Differences.	Bottom Thermometer.	
32°			32°	Differences.
		0		
In 10 min. 33+		1+	34+	2+
		2.5—		3—
30 — 35.5		1.5	37	1+
50 — 37		1	38+	0
1 hour — 38		4	38+	.25—
1 10 — 42		2	38.25	1.75
1 30 — 44			40	&c.

In the first interval of 10 minutes we observe the bottom thermometer to have gained 2°+, and the top only 1°+; the former has the heat which enters directly, together with the heat which descends by the side of the vessel; the latter has only the heat which enters directly, and as these are nearly as one to two, we may infer that the acquisition of direct heat, and heat by descent, are nearly equal in the bottom thermometer during that interval.

Inference that the water descends as it acquires heat,

In the next interval of 20 minutes we observe the bottom thermometer gains 3°—, and the top 2°5—. Here we see the

the ascending current still continues, but has produced little effect, having not added more than half a degree to the temperature.

—until the temperature rather exceeds 36° .

During the next 20 minutes the top gains $1^{\circ}.5$, the bottom only $1^{\circ}+$. In this interval we may observe the current has turned, but not yet acquired much force. The point of greatest density must therefore have existed at the last observation or near it; the mean of $35^{\circ}.5$ and 37° is $36\frac{1}{2}^{\circ}$ for the required point, as deduced from this experiment.

After which the heated water ascends.

In 10 minutes more the top gains 1° , and the bottom little or nothing; here we find the ascending current has become such as to manifest its influence very sensibly.

In the next 10 minutes the top gains 4° , and the bottom only .25; here the ascending current has become quadruple what it was 2° below; because the farther the temperature is raised above the stationary point, the more powerful is the force of ascent arising from the same interval of temperature.

These facts do not agree with the supposed maximum density at 39° or 40° .

It would be in vain to attempt to reconcile the above experiment with the opinion that water is densest at 39° or 40° . At the very moment when the mean temperature of the water is 39° , we observe the *ascending* current the most active, when it ought to have been *descending* or imperceptible.

The effect is not modified by the table or support.

I once imagined that the experiment might be explained on the supposition of 32° being the point of greatest density; that the sudden increase of temperature at the bottom arose from the heat of the table, and that the cohesion of the particles of water prevented their ascent under the propulsion of so small a force; but having procured a large glass jar which could be suspended, I found the same order of differences nearly as when placed on a table, and was therefore obliged to abandon that explanation.

Intending to send the remainder of this investigation for a future number, I remain

Your friend,

JOHN DALTON.

Manchester, April 14, 1806.

III.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Action of Heat. By SIR JAMES HALL, Bart. F. R. S. Edinburgh.

(Concluded from Page 328.)

SECTION III.

Experiments made in Tubes of Porcelain.—Tubes of Wedgwood's Ware.—Methods used to confine the carbonic Acid, and to close the Pores of the Porcelain in a horizontal Apparatus.—Tubes made with a View to these Experiments.—The vertical Apparatus adopted.—View of Results obtained, both in Iron and Porcelain.—The Formation of Lime-stone and Marble.—Inquiry into the Cause of the partial Calcinations.—Tubes of Porcelain weighed previous to breaking—Experiments with Porcelain Tubes proved to be limited.

WHILE I was carrying on the above-mentioned experiments, I was occasionally occupied with another set, in tubes of porcelain. So much, indeed, was I prepossessed in favour of this last mode, that I laid gun-barrels aside, and adhered to it during more than a year. The methods followed with this substance differ widely from those already described, though founded on the same general principles.

Set of experiments in tubes of porcelain.

I procured from Mr. Wedgwood's manufactory at Etruria, in Staffordshire, a set of tubes for this purpose, formed of the same substance with the white mortars, in common use, made there. These tubes were fourteen inches long, with a bore of half an inch diameter, and thickness of 0.2; being closed at one end (figs. 9, 10, 11, 12, 13.) Pl. XI.

I proposed to ram the carbonate of lime into the breech (Fig. 9. A); then filling the tube to within a small distance of its muzzle with pounded flint (B), to fill that remainder (C) with common borax of the shops (borat of soda) previously reduced to glass, and then pounded; to apply heat to the muzzle alone, so as to convert that borax into solid glass; then, reversing the operation, to keep the muzzle cold, and apply the requisite heat to the carbonate lodged in the breech.

They were closed at one end and the other aperture was stopped with glass of borax.

I thus expected to confine the carbonic acid; but the attempt was attended with considerable difficulty, and has led

Difficulties of this process.

to the employment of various devices, which I shall now shortly enumerate, as they occurred in the course of practice. The simple application of the principle was found insufficient, from two causes: First, The carbonic acid being driven from the breech of the tube, towards the muzzle, among the pores of the pounded filix, escaped from the compressing force, by lodging itself in cavities which were comparatively cold: Secondly, The glass of borax, on cooling, was always found to crack very much, so that its tightness could not be depended on.

And the method
of partly obvi-
ating them.

To obviate both these inconveniences at once, it occurred to me, in addition to the first arrangement, to place some borax (Fig. 10. C) so near the breech of the tube, as to undergo heat along with the carbonate (A); but interposing between this borax and the carbonate, a stratum of filix (B), in order to prevent contamination. I trusted that the borax in a liquid or viscid state, being thrust outwards by the expansion of the carbonic acid, would press against the filix beyond it (D), and totally prevent the elastic substances from escaping out of the tube, or even from wandering into its cold parts.

In some respects, this plan answered to expectation. The glass of borax, which can never be obtained when cold, without innumerable cracks, unites into one continued viscid mass in the lowest red-heat; and as the fires in these experiments begins only with redness, the borax being heated at the same time with the carbonate, becomes united and impervious, as soon as its action is necessary. Many good results were accordingly obtained in this way. But I found, in practice, that as the heat rose, the borax began to enter into too thin fusion, and was often lost among the pores of the filix, the space in which it had lain being found empty on breaking the tube. It was therefore found necessary to oppose something more substantial and compact, to the thin and penetrating quality of pure borax.

Bottle glass was
found much pre-
ferable to pure
borax for the
purpose of re-
straining the
carbonic acid.

In searching for some such substance, a curious property of bottle-glass occurred accidentally. Some of this glass, in powder, having been introduced into a muffle at the temperature of about 20° of Wedgwood; the powder, in the space of about a minute, entered into a state of viscid agglutination, like that of honey, and in about a minute more, (the heat always continuing unchanged), consolidated into a firm and com-

paste mass of *Reaumur's porcelain**. It now appeared, that by placing this substance immediately behind the borax, the penetrating quality of this salt might be effectually restrained; for, *Reaumur's porcelain* has the double advantage of being refractory, and of not cracking by change of temperature. I found, however, that in the act of consolidation, the pounded bottle-glass shrunk, so as to leave an opening between its mass and the tube, through which the borax, and, along with it, the carbonic acid, was found to escape. But the object in view was obtained by means of a mixture of pounded bottle-glass, and pounded flint, in equal parts. This compound still agglutinates, not indeed into a mass so hard as *Reaumur's porcelain*, but sufficiently so for the purpose; and this being done without any sensible contraction, an effectual barrier was opposed to the borax; (this arrangement is shewn in *Fig. 11.*); and thus the method of closing the tubes was rendered so complete, as seldom to fail in practice†. A still further refinement upon this method was found to be of advantage. A second series of powders, like that already described, was introduced towards the muzzle, (as shewn in *Fig. 12.*) During the first period of the experiment, this last-mentioned series was exposed to heat, with all the outward half of the tube (*ab*); and by this means, a solid mass was produced, which remained cold and firm during the subsequent action of heat upon the carbonate.

Improvements
on this method.

I soon found, that notwithstanding all the above-mentioned precautions, the carbonic acid made its escape, and that it pervaded the substance of the Wedgwood tubes, where no flaw could be traced. It occurred to me, that this defect might be remedied, were borax, in its thin and penetrating state of fusion, applied to the inside of the tube; and that the pores of the porcelain might thus be closed, as those of leather are closed by oil, in an air-pump. In this view, I rammed the carbonate into a small tube, and surrounded it with pounded glass of borax, which, as soon as the heat was applied, spread on the in-

Remedy for porosity in the earthen tubes.

* In the same temperature, a mass of the glass of equal bulk would undergo the same change; but it would occupy an hour.

† A substance equally efficacious in restraining the penetrating quality of borax, was discovered by another accident. It consists of a mixture of borax and common sand, by which a substance is formed, which, in heat, assumes the state of a very tough paste, and becomes hard and compact on cooling.

side

side of the large tube, and effectually closed its pores. In this manner, many good experiments were made with barrels lying horizontally in common muffle, (the arrangement just described being represented in *Fig. 13.*)

Best material for tubes.

I was thus enabled to carry on experiments with this porcelain, to the utmost that its strength would bear. But I was not satisfied with the force so exerted; and hoping to obtain tubes of a superior quality, I spent much time in experiments with various porcelain compositions. In this, I so far succeeded, as to produce tubes by which the carbonic acid was in a great measure retained without any internal glaze. The best material I found for this purpose, was the pure porcelain-clay of Cornwall, or a composition in the proportion of two of this clay to one of what the potters call *Cornish-stone*, which I believe to be a granite in a state of decomposition. These tubes were seven or eight inches long, with a bore tapering from 1 inch to 0.6. Their thickness was about 0.3 at the breech, and tapered towards the muzzle to the thinness of a wafer.

Improvement by placing the tube vertically.

I now adopted a new mode of operation, placing the tube vertically, and not horizontally, as before. By observing the thin state of borax whilst in fusion, I was convinced, that it ought to be treated as a complete liquid, which being supported in the course of the experiment from below, would secure perfect tightness, and obviate the failure which often happened in the horizontal position, from the falling of the borax to the lower side.

Particular description of the process.

In this view, (*fig. 16.*) I filled the breech in the manner described above, and introduced into the muzzle some borax (C) supported at the middle of the tube by a quantity of flint mixed with the bottle glass (B). I placed the tube, so prepared, with its breech plunged into a crucible filled with sand (E), and its muzzle pointing upwards. It was now my object to apply heat to the muzzle-half, whilst the other remained cold. In that view, I constructed a furnace (*figs. 14. and 15.*) having a muffle placed vertically (c d,) surrounded on all sides with fire (e e), and open both above (at c), and below (at d). The crucible just mentioned, with its tube, being then placed on a support directly below the vertical muffle, (as represented in *fig. 14.* at F) it was raised, so that the half of the tube next the muzzle was introduced into the

the fire. In consequence of this, the borax was seen from above to melt, and run down in the tube, the air contained in the powder escaping in the form of bubbles, till at last the borax stood with a clear and steady surface like that of water. Some of this salt being thrown in from above, by means of a tube of glass, the liquid surface was raised nearly to the muzzle, and, after all had been allowed to become cold, the position of the tube was reversed; the muzzle being now plunged into the sand, (as in fig. 17.) and the breech introduced into the muffle. In several experiments, I found it answer well, to occupy great part of the space next the muzzle, with a rod of sand and clay previously baked, (fig. 19. K K), which was either introduced at first, along with the pounded borax, or, being made red hot, was plunged into it when in a liquid state. In many cases I assisted the compactness of the tube by means of an internal glaze of borax; the carbonate being placed in a small tube, (as shewn in fig. 18.)

These devices answered the end proposed. Three-fourths of the tube next the muzzle was found completely filled with borax, having a concave termination at both ends, (f and g in figs. 17, 18, 19.) shewing that it had stood as a liquid in the opposite positions in which heat had been applied to it. A great degree of tightness indeed was obtained in this manner, as I found myself subjected to an unforeseen source of failure. A number of the tubes failed, not by explosion, but by a contraction of a minute longitudinal fissure at the breech, through which the borax and carbonic acid escaped. I saw that this arose from the expansion of the borax when in a liquid state, as happened with the fusible metal in the experiments with iron-barrels; for, the crevice here formed, indicated the exertion of some force acting very powerfully, and to a very small distance. Accordingly, this source of failure was remedied by the introduction of a very small air-tube. This, however, was used only in a few experiments.

Effect of expansion in the fused borax upon the tubes.

In the course of the years 1801, 1802, and 1803, I made a number of experiments, by the various methods above described, amounting, together with those made in gun barrels, to one hundred and fifty-six. In an operation so new, and in which the apparatus was strained to the utmost of its power, constant success could not be expected, and in fact many experiments failed, wholly or partially. The results, however,

These experiments were upon the whole successful.

upon

upon the whole, were satisfactory, since they seemed to establish some of the essential points of this inquiry.

These experiments prove, that, by mechanical constraint, the carbonate of lime can be made to undergo strong heat, without calcination, and to retain almost the whole of its carbonic acid, which, in an open fire, at the same temperature, would have been entirely driven off; and that, in these circumstances, heat produces some of the identical effects ascribed to it in the Huttonian Theory.

Pounded carbonate of lime in its several varieties became agglutinated into stony masses.

By this joint action of heat and pressure, the carbonate of lime which had been introduced in the state of the finest powder, is agglutinated into a firm mass, possessing a degree of hardness, compactness, and specific gravity* nearly approaching to these qualities in a sound limestone; and some of the results, by their saline fracture, by their semitransparency, and their susceptibility of polish, deserve the name of marble.

The same trials have been made with all calcareous substances; with chalk, common limestone, marble, spar, and the shells of fish. All have shewn the same general property, with some varieties as to temperature. Thus, I found, that, in the same circumstances, chalk was more susceptible of agglutination than spar; the latter requiring a heat two degrees higher than the former, to bring it to the same pitch of agglutination.

The chalk used in my first experiments, always assumed the character of a yellow marble, owing probably to some slight contamination of iron. When a solid piece of chalk, whose bulk had been previously measured in the gauge of Wedgwood's pyrometer was submitted to heat under compression, its contraction was remarkable, proving the approach of the particles during their consolidation; on these occasions, it was found to shrink three times more than the pyrometer-pieces in the same temperature. It lost, too, almost entirely, its power of imbibing water, and acquired a great additional specific gravity. On several occasions, I observed, that masses of chalk, which, before the experiment, had shewn one uniform character of whiteness, assumed a stratified appearance, indicated by a series of parallel layers of a brown colour. This

* See Appendix.

circumstance may hereafter throw light on the geological history of this extraordinary substance.

I have said, that, by mechanical constraint, almost the whole of the carbonic acid was retained. And, in truth, at this period, some 1-4 of weight had been experienced in all the experiments, both with iron and porcelain. But even this circumstance is valuable, by exhibiting the influence of the carbonic acid, as varied by its quantity.

And most of the carbonic acid was retained.

When the loss exceeded 10 or 15 per cent. * of the weight of the carbonate, the result was always of a friable texture, and without any stony character; when less than two or three per cent. it was considered as good, and possessed the properties of a natural carbonate. In the intermediate cases, when the loss amounted, for instance, to six or eight per cent. the result was sometimes excellent at first, the substance bearing every appearance of soundness, and often possessing a high character of crystallization; but it was unable to resist the action of the air; and, by attracting carbonic acid or moisture, or both, crumbled to dust more or less rapidly, according to circumstances. This seems to prove, that the carbonate of lime, though not fully saturated with carbonic acid, may possess the properties of limestone; and perhaps a difference of this kind may exist among natural carbonates, and give rise to their different degrees of durability.

Qualities of the product differing according to the loss of carbonic acid.

I have observed, in many cases, that the calcination has reached only to a certain depth into the mass; the internal part remaining in a state of complete carbonate, and, in general, of a very fine quality. The partial calcination seems thus to take place in two different modes. By one, a small proportion of carbonic acid is taken from each particle of carbonate; by the other, a portion of the carbonate is quite calcined, while the rest is left entire. Perhaps one result is the effect of a feeble calcining cause, acting during a long time, and the other of a strong cause, acting for a short time.

Some of the results which seemed the most perfect when first produced, have been subject to decay, owing to partial calcination. It happened, in some degree, to the beautiful

Some results were subject to decay from partial calcination.

* I have found, that, in open fire, the entire loss sustained by the carbonate varies in different kinds from 42 to 45.5 per cent.

specimen

specimen produced on the 3d of March, 1801, though a fresh fracture has restored it.

A specimen, too, of marble, formed from pounded spar, on the 15th of May, 1801, was so complete as to deceive the workman employed to polish it, who declared, *these* were the substance a little whiter, the quarry from which it was taken would be of great value, if it lay within reach of a market. Yet, in a few weeks after its formation, it fell to dust.

Very many
were durable
marble.

Numberless specimens, however, have been obtained, which resist the air, and retain their polish as well as any marble. Some of them continue in a perfect state, though they have been kept without any precaution during four or five years. That set, in particular, remain perfectly entire, which were shewn last year in this Society, though some of them were made in 1799, some in 1801 and 1802, and though the first eleven were long soaked in water, in the trials made of their specific gravity.

Remarkable
fact.

A curious circumstance occurred in one of these experiments, which may hereafter lead to important consequences. Some rust of iron had accidentally found its way into the tube: 10 grains of carbonate were used, and a heat of 29° was applied. The tube had no flaw; but there was a certainty that the carbonic acid had escaped through its pores. When broken, the place of the carbonate was found occupied, partly by a black slaggy matter, and partly by sphericles of various sizes, from that of a small pea downwards, of a white substance, which proved to be quicklime; the sphericles being interspersed through the slag, as spar and agates appear in whinstone. The slag had certainly been produced by a mixture of the iron with the substance of the tube; and the spherical form of the quicklime seems to shew, that the carbonate had been in fusion along with the slag, and that they had separated on the escape of the carbonic acid:

The subject was carried thus far in 1803, when I should probably have published my experiments, had I not been induced to prosecute the inquiry by certain indications, and accidental results, of a nature too irregular and uncertain to meet the public eye, but which convinced me, that it was possible to establish by experiment the truth of all that was hypothetically assumed in the Huttonian theory.

Endeavour to
improve the ex-
periments by

The principal object was now to accomplish the entire fusion of the carbonate, and to obtain spar as the result of that fusion.

fusion, in imitation of what we conceive to have taken place preventing all calcination, and perfectly fusing the carbonate.
in nature.

• It was likewise important to acquire the power of retaining all the carbonic acid of the carbonate, both on account of the fact itself, and on account of its consequences; the result being visibly improved by every approach towards complete saturation. I therefore became anxious to investigate the cause of the partial calcinations which had always taken place, to a greater or less degree, in all these experiments. The question naturally suggests itself, What has become of the carbonic acid, separated in these partial calcinations from the earthy basis? Has it penetrated the vessel, and escaped entirely, or has it been retained within it in a gaseous, but highly compressed state? It occurred to me, that this question might be easily resolved, by weighing the vessel before and after the action of heat upon the carbonate.

With iron, a constant and inappreciable source of irregularity existed in the oxidation of the barrel. But with porcelain the thing was easy; and I put it in practice in all my experiments with this material, which were made after the question had occurred to me. The tube was weighed as soon as its muzzle was closed, and again, after the breech had been exposed to the fire; taking care, in both cases, to allow all to cool. In every case, I found some loss of weight, proving, that even in the best experiments, the tubes were penetrated to a certain degree. I next wished to try if any of the carbonic acid separated, remained within the tube in a gaseous form; and in that view, I wrapt the tube, which had just been weighed, in a sheet of paper, and placed it, so surrounded, on the scale of the balance. As soon as its weight was ascertained, I broke the tube by a smart blow, and then replaced upon the scale the paper containing all the fragments. In those experiments, in which entire calcination had taken place, the weight was found not to be changed, for all the carbonic acid had already escaped during the action of heat. But in the good results, I always found that a loss of weight was the consequence of breaking the tube.

These facts prove, that both causes of calcination had operated in the porcelain tubes; that, in the cases of small loss, part of the carbonic acid had escaped through the vessel, and that part had been retained within it. I had in view methods of the acid
By experiment it was found that iron barrels vitiated the result.

by which the last could be counteracted; but I saw no remedy for the first. I began, therefore, to despair of ultimate success with tubes of porcelain*.

These last could not bear elevated heats.

Another circumstance confirmed me in this opinion. I found it impracticable to apply a heat of 27° to these tubes, when charged as above with carbonate, without destroying them, either by explosion, by the formation of a minute rent, or by the actual swelling of the tube. Sometimes this swelling took place to the amount of doubling the internal diameter, and yet the porcelain held tight, the carbonate sustaining but a very small loss. This ductility of the porcelain in a low heat is a curious fact, and shews what a range of temperature is embraced by the gradual transition of some substances from a solid to a liquid state: For the same porcelain, which is thus susceptible of being stretched out without breaking in a heat of 27° , stands the heat of 152° , without injury, when exposed to no violence, the angles of its fracture remaining sharp and entire.

IV.

Experiments in Gun-Barrels resumed.—The Vertical Apparatus applied to them.—Barrels bored in solid Bars.—Old Sable Iron.—Fusion of the Carbonate of Lime.—Its Action on Porcelain.—Additional Apparatus required in Consequence of that Action.—Good Results; in particular, four Experiments, illustrating the Theory of Internal Calcination, and shewing the Efficacy of the Carbonic Acid as a Flux.

Experiments with gun barrels resumed,

SINCE I found that, with porcelain tubes, I could neither confine the carbonic acid entirely, nor expose the carbonate in them to strong heats; I at last determined to lay them aside, and return to barrels of iron, with which I had formerly obtained some good results, favoured, perhaps, by some accidental circumstances.

* I am nevertheless of opinion, that, in some situations, experiments with compression may be carried on with great ease and advantage in such tubes. I allude to the situation of the geologists of France and Germany, who may easily procure, from their own manufactories, tubes of a quality far superior to any thing made for sale in this country.

On

On the 12th of February, 1803, I began a series of experiments with gun-barrels, resuming my former method of working with the fusible metal, and with lead; but altering the position of the barrel from horizontal to vertical; the breech being placed upward, being the action of heat on the carbonate. This very simple improvement has been productive of advantages no less remarkable, than in the case of the tubes of porcelain. In this new position, the included air, quitting the air-tube on the fusion of the metal, and rising to the breech, is exposed to the greatest heat of the furnace, and must therefore react with its greatest force; whereas, in the horizontal position, that air might go as far back as the fusion of the metal reached, where its elasticity would be much feebler. The same disposition enabled me to keep the muzzle of the barrel plunged, during the action of heat, in a vessel filled with water; which contributed very much both to the convenience and safety of these experiments.

In this view, making use of the brick-furnace with the vertical muffle, already described in page 384, I ordered a pit (*a a* fig. 20.) to be excavated under it, for the purpose of receiving a water-vessel. This vessel (represented separately, fig. 21.) was made of cast iron; it was three inches in diameter, and three feet deep; and had a pipe (*d e*) striking off from it at right angles, four or five inches below its rim, communicating with a cup (*e f*) at the distance of about two feet. The main vessel being placed in the pit (*a a*) directly below the vertical muffle, and the cup standing clear of the furnace, water poured into the cup flowed into the vessel, and could thus conveniently be made to stand at any level. (The whole arrangement is represented in fig. 20.) The muzzle of the barrel (*g*) being plunged into the water, and its breech (*b*) reaching up into the muffle, as far as was found convenient, its position was secured by an iron chain (*g f*). The barrel being heated downwards generally kept the surface of the water in a state of ebullition; the waste thus occasioned being supplied by means of the cup, into which, if necessary, a constant stream could be made to flow.

As formerly I rammed the carbonate into a tube of porcelain, and placed it in a cradle of iron, along with an air-tube and a pyrometer; the cradle being fixed to a rod of iron, which rod I now judged proper to make as large as the barrel

Description and drawing of the apparatus.

would admit, in order to exclude as much of the fusible metal as possible; for the expansion of the liquid metal being in proportion to the quantity heated, the more that quantity could be reduced, the less risk there was of destroying the barrels.

Simple mode of withdrawing the contents from the tube.

In the course of practice, a simple mode occurred of removing the metal and withdrawing the cradle: it consisted in placing the barrel with its muzzle downwards, so as to keep the breech above the furnace and cold, while its muzzle was exposed to strong heat in the muffle. In this manner, the metal was discharged from the muzzle, and the position of the barrel being lowered by degrees, the whole metal was removed in succession, till at last the cradle and its contents became entirely loose. As the metal was delivered, it was received in a crucible, filled with water, standing on a plate of iron placed over the pit, which had been used, during the first stage of the experiment, to contain the water-vessel. It was found to be of service, especially where lead was used, to give much more heat to the muzzle than simply what was required to liquefy the metal it contained; for when this was not done, the muzzle growing cold as the breech was heating, some of the metal delivered from the breech was congealed at the muzzle, so as to stop the passage.

According to this method, many experiments were made in gun-barrels, by which some very material steps were gained in the investigation.

Experiment in the new method.

On the 24th of February, I made an experiment with spar and chalk; the spar being placed nearest to the breech of the barrel, and exposed to the greatest heat, some baked clay intervening between the carbonates. On opening the barrel, a long-continued hissing noise was heard. The spar was in a state of entire calcination; the chalk, though crumbling at the outside, was uncommonly hard and firm in the heart. The temperature had risen to 32°.

Internal calcination, where the carbonic acid did not escape out of the apparatus.

In this experiment, we have the first clear example, in iron barrels, of what I call *Internal Calcination*; that is to say, where the carbonic acid separated from the earthy basis, has been accumulated in cavities within the barrel. For, subsequently to the action of strong heat, the barrel had been completely cooled; the air therefore introduced by means of the air-tube, must have resumed its original bulk, and by itself could have

have no tendency to rush out; the heat employed to open the barrel was barely sufficient to soften the metal. Since, the

opening of the barrel was accompanied by the discharge of elastic matter in great abundance, it is evident, that this must have proceeded from something superadded to the air originally included, which could be nothing but the carbonic acid of the carbonate. It follows, that the calcination had been, in part at least, internal; the separation of the acid from the earthy matter being complete where the heat was strongest, and only partial where the intensity was less.

The chemical principles stated in a former part of this paper, authorised us to expect a result of this kind. As heat, by increasing the volatility of the acid, tended to separate it from the earth, we had reason to expect, that, under the same compression, but in different temperatures, one portion of the carbonate might be calcined, and another not: And that the least heated of the two, would be the least exposed to a change not only from want of heat, but likewise in consequence of the calcination of the other mass; for the carbonic acid disengaged by the calcination of the hottest of the two, must have added to the elasticity of the confined elastic fluid, so as to produce an increase of compression. By this means, the calcination of the coldest of the two might be altogether prevented, and that of the hottest might be hindered from making any further advancement. This reasoning seemed to explain the partial calcinations which had frequently occurred where there was no proof of leakage; and it opened some new practical views in these experiments, of which I availed myself without loss of time. If the internal calcination of one part of an inclosed mass, promotes the compression of other masses included along with it, I conceived that we might forward our views very much by placing a small quantity of carbonate, carefully weighed, in the same barrel with a large quantity of that substance; and by arranging matters so that the small fiducial part should undergo a moderate heat, while a stronger heat, capable of producing internal calcination, should be applied to the rest of the carbonate. In this manner, I made many experiments, and obtained results which seemed to confirm this reasoning, and which were often very satisfactory, though the heat did not always exert its greatest force where I intended it to do so.

Part of the included carbonate was calcined, another part retaining its acid. Reasoning on this fact.

Experiment.
Carbonate partly
fused, and in
part deprived of
carbonic acid.

On the 28th of February, I introduced some carbonate, accurately weighed, into a small porcelain tube, placed within a larger one, the rest of the large tube being filled with pounded chalk; these carbonates, together with some pieces of chalk, placed along with the large tube in the cradle, weighing in all 195.7 grains. On opening the barrel, air rushed out with a long-continued hissing noise. The contents of the little tube were lost by the intrusion of some borax which had been introduced over the filix, in order to exclude the fusible metal. But the rest of the carbonate, contained in the large tube, came out in a fine state, being porous and frothy throughout; sparkling every where with facets, the angular form of which was distinguishable in some of the cavities by help of a lens; in some parts the substance exhibited the rounding of fusion; in many it was in a high degree transparent. It was yellow towards the lower end, and at the other almost colourless. At the upper end, the carbonate seemed to have united with the tube, and at the places of contact to have spread upon it; the union having the appearance of a mutual action. The general mass of carbonate entered into acid violently, but the thin stratum immediately contiguous to the tube, feebly, if at all.

Similar experiment, in which the carbonate exhibited more remarkable facets.

On the 3d of March, I introduced into a very clean tube of porcelain 36.8 of chalk. The tube was placed in the upper part of the cradle, the remaining space being filled with two pieces of chalk, cut for the purpose; the upper most of these being excavated, so as to answer the purpose of an air-tube. The pieces thus added, were computed to weigh about 300 grains. There was no pyrometer used; but the heat was guessed to be about 30° . After the barometer stood during a few minutes in its delivering position, the whole lead with the rod and cradle, were thrown out with a smart report, and with considerable force. The lowermost piece of chalk had scarcely been acted upon by heat. The upper part of the other piece was in a state of marble, with some remarkable facettes. The carbonate, in the little tube, had shrunk very much during the first action of heat, and had begun to sink upon itself, by a further advancement towards liquefaction. The mass was divided into several cylinders, lying confusedly upon each other; this division arising from the manner in which the pounded chalk was rammed into the tube in successive portions. In several

several places, particularly at the top, the carbonate was very porous, and full of decided air-holes, which could not have been formed but in a soft substance; the globular form and shining surface of all these cavities, clearly indicating fusion. The substance was semi-transparent; in some places yellow, and in some colourless. When broken, the solid parts shewed a saline fracture, composed of innumerable facettes. The carbonate adhered, from end to end, to the tube, and incorporated with it, so as to render it impossible to ascertain what loss had been sustained. In general, the line of contact was of a brown colour; yet there was no room for suspecting the presence of any foreign matter, except, perhaps, from the iron-rod which was used in ramming down the chalk. But, in subsequent experiments, I have observed the same brown or black colour at the union of the carbonate with the porcelain tubes, where the powder had been purposely rammed with a piece of wood; so that this colour, which has occurred in almost every similar case, remains to be accounted for. The carbonate effervesced violently with acid; the substance in contact with the tube, doing so, however, more feebly than in the heart, leaving a copious deposit of white sandy matter, which is doubtless a part of the tube, taken up by the carbonate in fusion.

On the 24th of March, I made a similar experiment, in a stout gun-barrel, and took some care, after the application of heat, to cool the barrel slowly, with a view to crystallization. The whole mass was found in a fine state, and untouched by the lead; having a semitransparent and saline structure, with various facettes. In one part, I found the most decided crystallization I had obtained, though of a small size: owing to its transparency it was not easily visible, till the light was made to reflect from the crystalline surface, which then produced a dazzle, very observable by the naked eye; when examined by means of a lens, it was seen to be composed of several plates, broken irregularly in the fracture of the specimen, all of which are parallel to each other, and reflect under the same angle so as to unite in producing the dazzle. This structure was observable equally well in both parts of the broken specimen. In a former experiment, as large a facette was obtained in a piece of solid chalk; but this result was of more consequence, as having been produced from chalk previously powdered.

The

The gun-barrels, though superior to porcelain were still too weak.

The foregoing experiments proved the superior efficacy of iron vessels over those of porcelain, even where the thickness was not great; and I persevered in making a great many experiments with gun-barrels, by which I occasionally obtained very fine results: but I was at last convinced, that their thickness was not sufficient to ensure regular and steady success. For this purpose, it appeared proper to employ vessels of such strength, as to bear a greater expansive force than was just necessary; since, occasionally, (owing to our ignorance of the relation between the various forces of expansion, affinity, tenacity, &c.) much more strain has been given to the vessels than was requisite. In such cases, barrels have been destroyed, which, as the results have proved, had acted with sufficient strength during the first stages of the experiments, though they had been unable to resist the subsequent overstrain. Thus, my success with gun-barrels, depended on the good fortune of having used a force no more than sufficient, to constrain the carbonic acid, and enable it to act as a flux on the lime. I therefore determined to have recourse to iron barrels of much greater strength, and tried various modes of construction.

Barrels formed by boring in solid bars of iron which proved excellent.

I had some barrels executed by wrapping a thick plate of iron round a mandrel, as is practised in the formation of gun-barrels; and likewise by bringing the two flat sides together, so as to unite them by welding. These attempts, however, failed. I next thought of procuring bars of iron, and of having a cavity bored out of the solid, so as to form a barrel. In this manner I succeeded well. The first barrel I tried in this way was of small bore, only half an inch: Its performance was highly satisfactory, and such as to convince me, that the mode now adopted was the best of any that I had tried. Owing to the smallness of the bore, a pyrometer could not be used internally, but was placed upon the breech of the barrel, as it stood in the vertical muffle. In this position, it was evidently exposed to a much less heat than the fiducial part of the apparatus, which was always placed, as nearly as could be guessed, at the point of greatest heat.

Finely levigated spar became agglutinated by heat; semi-transparent, vitreous, with a few facettes.

On the 4th of April, an experiment was made in this way with some spar; the pyrometer on the breech giving 33° . The spar came out clean, and free from any contamination, adhering to the inside of the porcelain tube: it was very much

shrunk, still retaining a cylindrical form, though bent by partial adhesions. Its surface bore scarcely any remains of the impression taken by the powder, on ramming it into the tube: it had, to the naked eye, the roughness and semitransparency of the pith of a rush stripped of its outer skin. By the lens, this same surface was seen to be glazed all over, though irregularly, shewing here and there some air-holes. In fracture, it was semitransparent, more vitreous than crystalline, though having a few facettes: the mass, was seemingly formed of a congeries of parts, in themselves quite transparent: and, at the thin edges, small pieces were visible of perfect transparency. These must have been produced in the fire; for the spar had been ground with water; and passed through sieves, the same with the finest of those used at Etruria, as described by Mr. Weugwood, in his paper on the construction of his pyrometer.

With the same barrel I obtained many interesting results, giving as strong proofs of fusion as in any former experiments; with this remarkable difference, that, in these last, the substance was compact, with little or no trace of frothing. In the gun-barrels where fusion had taken place, there had always been a loss of 4 or 5 per cent. connected, probably, with the frothing. In these experiments, for a reason soon to be stated, the circumstance of weight could not be observed; but appearances led me to suppose, that here the loss had been small, if any.

In these experiments the escape of carbonic acid appears to have been less.

On the 6th of April, I made another experiment with the square barrel, whose thickness was now much reduced by successive scales, produced by oxidation, and in which a small rent began to appear externally, which did not, however, penetrate to the bore. The heat rose high, a pyrometer on the breech of the barrel giving 37° . On removing the metals, the cradle was found to be fixed, and was broken in the attempts made to withdraw it. The rent was much widened externally: but it was evident, that the barrel had not been laid open, for part of the carbonate was in a state of saline marble; another was hard and white, without any saline grains, and scarcely effervesced in acid. It was probably quicklime, formed by internal calcination, but in a state that has not occurred in any other experiment.

Gradual failure of the barrel bored from the bottom

Remarkable
fact of crystals
which appear to
have been formed
by sublimation

The workman whom I employed to take out the remains of the cradle, had cut off a piece from the breech of the barrel, three or four inches in length. As I was examining the crack which was seen in this piece, I was surprised to see the inside of the barrel lined with a set of transparent and well-defined crystals, of small size, yet visible by the naked eye. They lay together in some places, so as to cover the surface of the iron with a transparent coat; in others they were detached, and scattered over the surface. Unfortunately, the quantity of this substance was too small to admit of much chemical examination; but I immediately ascertained, that it did not in the least effervesce in acid, nor did it seem to dissolve in it. The crystals were in general transparent and colourless, though a few of them were tinged seemingly with iron. Their form was very well defined, being flat, with oblique angles, and bearing a strong resemblance to the crystals of the Lamellated Stylbite of Haüy. Though made above two years ago, they still retain their form and transparency unchanged. Whatever this substance may be, its appearance, in this experiment, is in the highest degree interesting, as it seems to afford an example of the mode in which Dr. Hutton supposes many internal cavities to have been lined, by the sublimation of substances in a state of vapour; or, held in solution, by matters in a gaseous form. For, as the crystals adhered to a part of the barrel, which must have been occupied by air during the action of heat, it seems next to certain that they were produced by sublimation.

The old Sable
Siberian iron is
very tough at
high heats.

The very powerful effects produced by this last barrel, the size of which (reduced, indeed, by repeated oxidation) was not above an inch square, made me very anxious to obtain barrels of the same substance, which being made of greater size, ought to afford results of extreme interest. I found upon inquiry, that this barrel was not made of Swedish iron, as I at first supposed, but of what is known by the name of *Old Sable*, from the figure of a Sable stamped upon the bars; that being the armorial badge of the place in Siberia where this iron is made.*

All iron is
crushed under
the hammer at
some degree
heat. Cast iron
at a low heat;

A workman explained to me some of the properties of different kinds of irons, most interesting in my present pursuit; and

* I was favoured with this account by the late Professor Robison.

he

he illustrated what he said by actual trial. All iron, when exposed to a certain heat, crushes and crumbles under the hammer; but the temperature in which this happens, varies with every different species. Thus as he shewed me, cast iron crushes in a dull red heat, or perhaps about 15° of Wedgwood; steel, in a heat perhaps at 30° ; Swedish iron, in a bright white heat, perhaps of 50° or 60° ; old Sable itself, likewise yields, but in a much higher heat, perhaps of 100° . I merely guessed at these temperatures; but I am certain of this, that in a heat similar to that in which Swedish iron crumbled under the hammer, the old Sable withstood a strong blow, and seemed to possess considerable firmness. It is from a knowledge of this quality, that the blacksmith, when he first takes his iron from the forge, and lays it on the anvil, begins by very gentle blows, till the temperature has sunk to the degree in which the iron can bear the hammer. I observed, as the strong heat of the forge acted on the Swedish iron, that it began to boil at the surface, clearly indicating the discharge of some gaseous matter; whereas, the old Sable, in the same circumstances, acquired the shining surface of a liquid, and melted away without any effervescence. I procured, at this time, a considerable number of bars of that iron, which fully answered my expectations.

By the experiments last mentioned, a very important point was gained in this investigation; the complete fusibility of the carbonate under pressure being thereby established. But from this very circumstance, a necessity arose of adding some new devices to those already described; for the carbonate, in fusion, spreading itself on the inside of the tube containing it, and the two uniting firmly together, so as to be quite inseparable, it was impossible, after the experiment, to ascertain the weight of the carbonate by any method previously used. I therefore determined in future to adopt the following arrangement.

A small tube of porcelain (*ik*, Fig. 23.) was weighed by means of a counterpoise of sand, or granulated tin; then the carbonate was firmly rammed into the tube, and the whole weighed again: thus the weight of the carbonate, previous to the experiment was ascertained. After the experiment, the tube, with its contents, was again weighed; and the variation of weight obtained, independently of any mutual action that had taken

steel at a higher, Swedish iron at a bright white heat, and old Sable at a still higher temperature.

The complete fusibility of the carbonate under pressure was ascertained in these strong barrels.

An arrangement for obviating some difficulties which arose from the fusion. The carbonate was put into a small porcelain tube and this properly secured in a larger. These

were placed in a (taken place between the tube and the carbonate. The balance cradle or frame which I used, turned in a constant and steady manner, with and the whole one hundredth of a grain. When pounded chalk was rammed put into the iron barrel, &c. into this tube, I generally left part of it free, and in that

space laid a small piece of lump-chalk (*i*), dressed to a cylinder, with the ends cut flat and smooth, and I usually cut a letter on each end, the more effectually to observe the effects produced by heat upon the chalk; the weight of this piece of chalk being always estimated along with that of the powder contained in the tube. In some experiments, I placed a cover of porcelain on the muzzle of the little tube, (this cover being weighed along with it), in order to provide against the case of ebullition: but as that did not often occur, I seldom took the trouble of this last precaution.

Continuation of
the method of
experiment.

It was now of consequence to protect the tube, thus prepared, from being touched during the experiment, by any substance, above all, by the carbonate of lime, which might adhere to it, and thus confound the appreciation by weight. This was provided for as follows: The small tube (*Fig. 23, i k*) with its pounded carbonate (*k*), and its cylinder of lump-chalk (*i*), was dropt into a large tube of porcelain (*p k*, *Fig. 24*). Upon this a fragment of porcelain (*l*), of such a size as not to fall in between the tubes, was laid. Then a cylinder of chalk (*m*) was dressed, so as nearly to fit and fill up the inside of the large tube, one end of it being rudely cut into the form of a cone. This mass being then introduced, with its cylindrical end downwards, was made to press upon the fragment of porcelain (*l*). I then dropped into the space (*n*), between the conical part of this mass and the tube, a set of fragments of chalk, of a size beyond what could possibly fall between the cylindrical part and the tube, and pressed them down with a blunt tool, by which the chalk being at the same time crushed and rammed into the angle, was forced into a mass of some solidity, which effectually prevented any thing from passing between the large mass of chalk and the tube. In practice, I have found this method always to answer, when done with care. I covered the chalk, thus rammed, with a stratum of pounded flint (*o*), and that again with pounded chalk (*p*) firmly rammed. In this manner, I filled the whole of the large tube with alternate layers of flint and chalk; the muzzle being always occupied with chalk, which was easily pressed into a mass of tolerable firmness.

firmness, and, suffering no change in very low heats, excluded the fusible metal in the first stages of the experiment.

Continuation of
the method of
experiment.

The large tube, thus filled, was placed in the cradle, sometimes with the muzzle upwards, and sometimes the reverse. I have frequently altered my views as to that part of the arrangement, each mode possessing peculiar advantages and disadvantages. With the muzzle upwards, (as shewn in *Fig. 24* and *25*), the best security is afforded against the intrusion of the fusible metal; because the air, quitting the air-tube in the working position, occupies the upper part of the barrel; and the fusible metal stands as a liquid (at *g*, *Fig. 25*.) below the muzzle of the tube, so that all communication is cut off, between the liquid metal and the inside of the tube. On the other hand, by this arrangement, the small tube, which is the fiducial part of the apparatus, is placed at a considerable distance from the breech of the barrel, so as either to undergo less heat than the upper part, or to render it necessary that the barrel be thrust high into the muzzle.

With the muzzle of the large tube downwards, the inner tube is placed (as shewn in *Fig. 22*), so as still to have its muzzle upwards, and in contact with the breech of the large tube. This has the advantage of placing the small tube near to the breech of the barrel: and though there is here less security against the intrusion of liquid metal, I have found that a point of little consequence; since, when the experiment is a good one, and that the carbonic acid has been well confined, the intrusion seldom takes place in any position. In whichever of the two opposite positions the large tube was placed, a pyrometer was always introduced, so as to lie as near as possible to the small tube. Thus, in the first-mentioned position, the pyrometer was placed immediately below the large tube, and, in the other position above it; so that, in both cases, it was separated from the carbonate by the thickness only of the two tubes.

Much room was unavoidably occupied by this method, which necessarily obliged me to use small quantities of carbonate, the subject of experiment seldom weighing more than 10 or 12 grains, and in others far less*.

On

* I measured the capacity of the air-tubes by means of granulated tin, acting as a fine and equal sand. By comparing the weight of

ACTION OF HEAT MODIFIED

Experiment made with the foregoing precautions. The heat amounted to 64° Wedgwood. The carbonates had lost gas and undergone fusion.

On the 11th of April, 1803, with a barrel of old sable iron having a bore of 0.75 of an inch, I made an experiment in which all these arrangements were put in practice. The large tube contained two small ones; one filled with spar, and the other with chalk. I conceived that the heat had risen to 33° , or somewhat higher. On melting the metals, the cradle was thrown out with considerable violence. The pyrometer, which, in this experiment, had been placed within the barrel, to my astonishment, indicated 64° . Yet all was found. The two little tubes came out quite clean and uncontaminated. The spar had lost 17.0 *per cent.* the chalk 10.7 *per cent.* The spar was half sunk down, and run against the side of the little tube: Its surface was shining, its texture spongy, and it was composed of a transparent and jelly-like substance. The chalk was entirely in a state of froth. This experiment extends our power of action, by shewing, that compression, to a considerable degree, can be carried on in so great a heat as 64° . It seems likewise to prove, that, in some of the late experiments with the square barrel, the heat had been much higher than was supposed at the time, from the indication of the pyrometer placed on the breech of the barrel; and that in some of them, particularly in the last, it must have risen at least as high as in the present experiment.

Experiment in which the barrel failed after its contents had undergone fusion.

On the 21st of April, 1805, a similar experiment was made with a new barrel, bored in a square bar of old sable, of about two and a half inch in diameter, having its angles merely rounded; the inner tube being filled with chalk. The heat was maintained during several hours, and the furnace allowed to burn out during the night. The barrel had the appearance of soundness, but the metals came off quietly, and the carbonate was entirely calcined, the pyrometer indicating 63° . On examination, and after beating off the smooth and even scale of oxide peculiar to the old sable, the barrel was found to have yielded in its peculiar manner; that is, by the opening of the longitudinal fibres. This experiment, notwithstanding the failure of the barrel, was one of the most interesting I had

of this tin with an equal bulk of water, I found that a cubic inch of it weighed 1330.6 grains, and that each grain of it corresponded to 0.00075 of a cubic inch. From these data I was able, with tolerable accuracy, to gage a tube by weighing the tin required to fill it.

made:

made, since it afforded proof of complete fusion. The carbonate had boiled over the lips of the little tube, standing, as just described, with its mouth upwards, and had run down to within half an inch of its lower end: most of the substance was in a frothy state, with large round cavities, and a shining surface; in other parts, it was interspersed with angular masses, which have evidently been surrounded by a liquid in which they floated. It was harder, I thought, than marble; giving no effervescence, and not turning red like quicklime in nitric acid, which seemed to have no effect upon it in the lump. It was probably a compound of quicklime with the substance of the tube.

With the same barrel repaired, and with others like it, many similar experiments were made at this time with great success; but to mention them in detail, would amount nearly to a repetition of what has been said. I shall take notice of only four of them, which, when compared together, throw much light on the theory of these operations, and likewise seem to establish a very important principle in geology. These four experiments differ from each other only in the heat employed, and in the quantity of air introduced.

The first of these experiments was made on the 27th of April 1803, in one of the large barrels of old sable, with all the above-mentioned arrangements. The heat had risen, contrary to my intention, to 78° and 79° . The tubes came out uncontaminated with fusible metal, and every thing bore the appearance of soundness. The contents of the little tube, consisting of pounded chalk, and of a small piece of lump-chalk, came out clean, and quite loose, not having adhered to the inside of the tube in the smallest degree. There was a loss of 41 per cent. and the calcination seemed to be complete; the substance, when thrown into nitric acid, turning red, without effervescence at first, though, after lying a few minutes, some bubbles appeared. According to the method followed in all these experiments, and lately described at length, (and shewn in *Fig. 21 and 25*), the large tube was filled over the small one, with various masses of chalk, some in lump, and some rammed into it in powder; and in the cradle there lay some pieces of chalk, filling up the space, so that in the cradle there was a continued chain of carbonate of four or five inches in length. The substance was found to be less and less calcined, the more

Account of some experiments at very elevated heats. The carbonate most heated was calcined; that which had suffered less heat had the form of lime-stone and of marble, which retained their carbonic acid.

it was removed from the breech of the barrel, where the heat was greatest. A small piece of chalk, placed at the distance of half an inch from the small tube, had some saline substance in the heart, surrounded and intermixed with quicklime, distinguished by its dull white. In nitric acid, this substance became red, but effervesced pretty briskly; the effervescence continuing till the whole was dissolved. The next portion of chalk was in a firm state of limestone; and a lump of chalk in the cradle, was equal in perfection to any marble I have obtained by compression: the two last-mentioned pieces of chalk effervescing with violence in the acid, and shewing no redness when thrown into it. These facts clearly prove, that the calcination of the contents of the small tube had been internal, owing to the violent heat which had separated its acid from the most heated part of the carbonate, according to the theory already stated. The soundness of the barrel was proved by the complete state of those carbonates which lay in less heated parts. The air-tube in this experiment had a capacity of 0.29, nearly one-third of a cubic inch.

Another experiment in which the barrel failed.

The second of these experiments was made on the 29th of April, in the same barrel with the last, after it had afforded some good results. The air-tube was reduced to one-third of its former bulk, that is, to one-tenth of a cubic inch.—The heat rose to 60°. The barrel was covered externally with a black spongy substance, the constant indication of failure, and a small drop of white metal made its appearance. The cradle was removed without any explosion or lifting. The carbonates were entirely calcined. The barrel had yielded, but had resisted well at first; for the contents of the little tube were found in a complete state of froth, and running with the porcelain.

Third experiment, very thin fusion.

The third experiment was made on the 30th of April, in another similar barrel. Every circumstance was the same as in the two last experiments, only that the air-tube was now reduced to half its last bulk, that is, to one-twentieth of a cubic inch. A pyrometer was placed at each end of the large tube. The uppermost gave 41°, the other only 35°. The contents of the inner tube had lost 16 per cent. and were reduced to a most beautiful state of froth, not very much injured by the internal calcination and indicating a thinner state of fusion than had appeared.

The fourth experiment was made on the 2d of May, like the rest in all respects, with a still smaller air-tube, of 0.0318, being less than one thirtieth of a cubic inch. The upper pyrometer gave 25° , and the under one 16° : The lowest masses of carbonate were scarcely affected by the heat. The contents of the little tube lost 2.9 per cent. both the lump and the pounded chalk were in a fine saline state, and, in several places, had run and spread upon the inside of the tube, which I had not expected to see in such a low heat. On the upper surface of the chalk rammed into the little tube, which, after its introduction had been wiped smooth, were a set of white crystals, with shining facettes, large enough to be distinguished by the naked eye, and seeming to rise out of the mass of carbonate. I likewise observed, that the solid mass on which these crystals stood, was uncommonly transparent.

In these four experiments, the bulk of the included air was successively diminished, and by that means its elasticity increased. The consequence was, that in the first experiment, where that elasticity was the least, the carbonic acid was allowed to separate from the lime, in an early stage of the rising heat, lower than the fusing point of the carbonate, and complete internal calcination was effected. In the second experiment, the elastic force being much greater, calcination was prevented, till the heat rose so high as to occasion the entire fusion of the carbonate, and its action on the tube, before the carbonic acid was set at liberty by the failure of the barrel. In the third experiment, with still greater elastic force, the carbonate was partly calcined, and its fusion accomplished, in a heat between 41° and 15° . In the last experiment, where the force was strongest of all, the carbonate was almost completely protected from decomposition by heat, in consequence of which it crystallized and acted on the tube, in a temperature between 25° and 16° . On the other hand, the efficacy of the carbonic acid as a flux on the lime, and in enabling the carbonate to act as a flux on other bodies, was clearly evinced; since the first experiment proved that quicklime by itself could neither be melted, nor act upon porcelain, even in the violent heat of 79° ; whereas, in the last experiment where the carbonic acid was retained, both of these effects took place in a very low temperature.

(To be continued.)

IV.

*Observations on the Effect of Madder Root on the Bones of Animals. By Mr. B. GIBSON.**

Account of the first discovery of the property of madder to tinge the bones of living animals.

THERE is, perhaps, no phenomenon, which occurs in an animal body more curious, than the tinge communicated to the bones of living animals, whose food has been mixed with madder root. This, like many other facts, to which no reasoning *a priori* could have directed us, was discovered by chance. Mr. Belcher, dining with a calico printer on a leg of fresh pork, was surprized that the bones, instead of possessing their usual whiteness, were of a deep red colour; and on enquiring the cause of it, was informed, that the pig had been fed upon the refuse of the dyers' vats, and had received so much of the colouring matter of madder into the system, that its bones were dyed by it. So interesting a fact has attracted very much the attention of anatomists, and has been used in many physiological and pathological enquiries; it may not therefore be uninteresting to give a short history of the phenomena connected with it, and the purposes to which it has been applied, previous to entering upon the more immediate object of this paper.

Experiments shewing that the tinge is more quickly given to the bones of growing animals.

Many experiments have been made to ascertain how long a time is required to produce the tinge, and whether it be permanent or only temporary. Belcher and Morand, about the same time, mixed madder root with the food of chickens and young pigeons. The result of their observations was,

Short time and other facts.

that the tinge was more quickly communicated to the bones of growing animals, than to the bones of animals which had already completed their growth; the bones of young pigeons being tinged of a rose-colour in twenty-four hours, and of a deep scarlet in three days; whilst the bones of adult animals only exhibited a rose-colour in fifteen days. They found the tinge most intense in the solid parts of those bones, which were nearest to the centre of circulation; whilst in bones of equal solidity, at a greater distance from the heart, the tint was more faint. The dye was deep in proportion to the length of time the madder had been continued, and when it was discontinued, the colour gradually became more and more faint, till

it entirely disappeared. According to the experiments of these gentlemen, other vegetable dyes, such as logwood, turmeric and alkanet root, did not communicate their respective tints to the bones.*

This effect of madder upon the bones, was soon afterwards made use of by Du Hamel, in his attempt to prove the manner in which the bones of animals are increased in thickness. Du Hamel used this property to shew the growth of bones. Observing in the vegetable kingdom, that the bark, by a sort of secretion, formed the ligneous part of a tree, in successive layers; so he conceived that the periosteum, or membrane surrounding bones, being converted into osseous matter, increased their diameter by adding to them concentric laminæ in succession. In order to prove the justness of his opinion, he mixed the food of a cock with madder root for a month, withheld it for a month, and then gave it again. He afterwards killed the animal, and upon inspection thought he observed the appearance which he expected; viz. two layers of red bone inclosing one of white, corresponding to the periods of the madder's being given or withheld.

This experiment, and some others related by Du Hamel, It is very doubtful whether that growth could be so indicated. appear to be conclusive in favour of the theory, which he wished to establish; and as they were conducted by a physiologist of high character, the accuracy of the observations could not have been doubted, had these experiments stood alone. But when they are compared with some of his own previous experiments, and those of other authors, it is difficult to reconcile them. In some of Du Hamel's experiments, for instance, the bones of a cock were tinged of a rose-colour through their whole substance in sixteen days, and those of young pigeons of a deep scarlet in three days. In several ex-

• • From some experiments I made on young pigeons, I found that a considerable quantity of logwood, in the form of extract, communicated an evidently purple tint to the bones. With regard to turmeric, it appears to be altered in its colour by passing through the digestive organs, for the excreta of the animals, who took it in considerable quantity, were constantly green: whilst either logwood or madder root exhibited their respective hues after passing through the intestines. Saffron exhibits properties different from any of these substances; for though a pigeon took it in considerable quantity, and thereby had its excreta tinged, yet no perceptible alteration of colour was produced in its bones. •

ON THE EFFECT OF MADDER ROOT

periments I have made on the subject, I have found the bones of young pigeons tinged of a uniform rose-colour, internally as well as externally, in twenty-four hours. This communication of colour to the whole substance of the osseous system in so short a time, makes it highly improbable, that the laminated appearance, remarked by Du Hamel, was produced by the new formation of red and white osseous layers, corresponding to the times (months) the madder had been given or withheld. For, as Mr. John Bell very justly remarks,* "If a bone should increase by layers thick enough to be visible and of a distinct tint, and such layers be continually accumulated upon each other every week, what kind of bone should this grow to?" The only way in which we can reconcile with each other the phenomena observed in the different experiments, and account for their apparent contradiction, is, by supposing that Du Hamel mistook for an obscurely laminated appearance, the variety in the tint, which is more deeply communicated to the more solid, and more faintly to the less compact parts of a bone.

Late experiments of Dr. M'Donald on the bones.

This property of madder of tinging the bones of animals, has lately been employed by Dr. M'Donald,† in his ingenious researches into the formation and death of bones. Amongst other objects, he attempted to ascertain in what manner and how soon a cylindrical bone is regenerated to supply the place of one artificially killed. As the process is highly curious, I shall briefly relate the principal points.

Very curious process of a bone destroyed,

Dr. M'Donald's experiments were made by amputating the proper leg-bone of young pigeons or chickens immediately above the joint. The marrow was then extracted, and the cavity which contained it, filled with lint. This process caused the death of the bone, and the formation of a new bone surrounding that destroyed ensued. Immediately after the experiment, the animal had its food mixed with madder root, and the part was inspected in different animals, at different periods.

—and the regular process of

On examination three days afterwards, the periosteum or enveloping membrane, was found much thickened; and underneath it a gelatinous humour was effused, surrounding the

* Anatomy of the bones, &c. p. 15.

† Disputatio inauguralis de Necrosi ac Callo. 1799.

dead bone, and spotted with red osseous nuclei; proving that the regeneration of the bone had commenced at this early period.

In seven days the new bone was found soft and flexible, not ^{—its regenera-} to be distinguished from cartilage or gristle, except by the red tint the madder had communicated to it; yet the bone destroyed was not at all coloured, although the other bones of the animal had acquired a bright red. From this time the new bone continued to encrease in hardness, surrounding the old one like a sheath. The latter in about three weeks was so loose as to be drawn out, and in about fifteen days from this time, the cavity of the regenerated bone was filled with marrow, and in every respect performed the office of that for which it was a substitute. This may be considered as a general outline of the progressive changes which take place during the regeneration of a cylindrical bone, in a young animal, such as a pigeon, or chicken; and the same process is frequently performed in the human body, when, from some internal cause, the life of a bone is destroyed. These changes involve many interesting particulars; but the circumstance most immediately connected with the subject of this paper is, that although the shaft of the bone required three weeks for its renewal, yet in seven days the osseous system generally had acquired a bright red. Now if we explain this change in colour according to the common opinion of absorption of the white, and deposition of the red osseous matter,* we must necessarily draw this conclusion; that the osseous system of the animal will be renewed three times during the period, which the formation of the substitute bone requires; a conclusion which we should be inclined to reject merely from its improbability. But besides this, the appear-

—its regenera-
tion.

Inference.
From the very
speedy acqui-
sition and subse-
quent loss of the
red colour that
the osseous sys-
tem was natural-
ly absorbed and
renewed in that
period.

* The common opinion of physiologists, with regard to this curious fact, is, that when a bone becomes red, during the exhibition of madder root, the white osseous particles which composed it, have been entirely removed by absorption and replaced by new osseous matter of a red colour: and when a bone assumes its natural colour, these red particles have been removed and replaced by white. If this be the fact, it necessarily follows, that an animal has at least fifty-two new sets of bones in a year: for the osseous system, according to the experiments of the most respectable physiologists, acquires a deep red tint from madder in one week, and assumes its natural colour in another.

ance of the parts strongly militate against it—for, if we may judge at all of the activity of the process in the two parts, by their comparative degrees of vascularity, that employed in forming the substitute bone far exceeds that going on in the osseous system generally; one striking phenomenon attending the regeneration of a bone being, the very high degree of increased vascularity which the parts employed in the process rapidly assume.

The bones are alone reddened by madder, because the phosphate of lime acts as a mordent on the madder.

After this effect of madder upon the bones was known, it long remained a mystery, why some other white parts of the body, such as nerves, cartilages and periosteum, were not equally liable to be coloured by it, as the bones. This fact, I believe, did not receive any explanation, until Dr. Rutherford gave a very ingenious and satisfactory one. When speaking of this property of madder, he says,* “We have, in the fact before us, a beautiful example of a particular case of chemical attraction; such as in numberless instances, is observed to take place between the colouring particles of both animal and vegetable substances and various other bodies, especially earths and earthy salts, and oxides of metals. So strong is the affinity of the colouring matter to these bodies, that it is frequently observed to quit the menstruum, in which it may chance to be dissolved, to unite with them: they, in consequence of its union, acquiring a particular tinge, whilst the menstruum is proportionably deprived of colour. From this principle, this mutual attraction, is deduced the various use of those bodies as mordents, as they are called, *intermedia*, or means for fixing the colours in dyeing or staining thread or cloth, whether it be composed of animal or vegetable materials. Upon the same principle depends the preparation of those pigments known to painters under the name of lakes; these are truly precipitates of the colouring matter, in combination with various mordents, as their basis.—The colouring of the bones of a living animal by means of madder, is, in every circumstance, analogous to the formation of these lakes. The colouring matter of madder, passing unaltered through the digestive organs of the animal, enters the general mass of fluids, and is dissolved in the serum of the blood, to which,

The red matter is a kind of lake.

* See Dr. Blake's inaugural Dissertation. De dentium formatione, p. 112.—1798.

indeed,

indeed, if it be in large proportion, it communicates a sensibly red tinge. But there is always present in the blood, and in the state of solution in the serum, a quantity of the earthy matter of the bones, phosphate of lime, ready to be deposited, as the exigencies of the animal may require. Now the phosphate of lime is an excellent mordant to madder, and has a strong affinity to it, and is consequently admirably fitted to afford a base for the colouring matter of it: in such experiments, therefore, they concrete in the state of a bright red lake, whence the colour of the bones is derived. That this is actually the case, may be shewn by a variety of experiments. Thus, if to an infusion of madder in distilled water, be added a little of the muriate of lime, no change is perceived: but if to this mixture be added a solution of the phosphate of soda, immediately a double elective attraction takes place. The muriatic acid combining with the soda, remains suspended, or dissolved in the water; whilst the phosphoric acid, thus deprived of its soda, combines with the lime which the muriatic acid parted with, and forms phosphate of lime or earth of bones. This substance however, being insoluble in water, falls to the bottom; but having combined at the instant of its formation with the colouring matter of the madder, they fall down united into a crimson lake; precisely of the same tint with that of the bones of young animals, which have been fed with madder. From this simple representation of the matter, we have a ready explication of every circumstance which has been remarked as extraordinary respecting this subject."

Whilst Dr. Rutherford thus gives a most satisfactory explanation of the colour of madder being communicated to the bones alone, of all the white parts of an animal; we find that he embraces the same opinion as other physiologists, that the osseous materials acquire their colour previous to their deposition, whilst in a state of solution or mixture in the blood; from whence they are afterwards deposited, and concrete in the form of a bright lake. In no part of his ingenious remarks does he hint at the probability that the bones already formed in an animal, may, during the use of madder, become red, and after its disuse gradually resume their natural colour, by the agency of a power entirely independent of their deposition and absorption: that this is probable I shall now proceed to prove.

Dr. Rutherford admits the absorption and deposition.

Before

More particular explanation of the doctrine of the absorption and regeneration of the parts of animals,

Before it was discovered that madder possessed this property of tinging bones, physiologists had long been of opinion, that the various parts of the body, being worn out by the performance of their actions and functions, were gradually removed, and replaced by new materials. They had seen, as Mr. J. Bell observes, the whole osseous system by the morbid removal of its solid part, rendered so soft and flexible as to bend under the common weight of the body and ordinary action of parts; the regeneration of many bones which had been destroyed by disease; the rapid absorption of fat in some diseases, and its speedy reproduction; and lastly, the gradual change which the fluids of the body undergo, as well as some of its insensible parts, the hair and nails; hence they supposed that the same process of change and renovation went on in every organ, and that the bodies of animals were not composed of the same identical particles of which they would consist at

—supposed to be confirmed in the bones.

some future period. This process, which was before but conjectural, or supported by analogy, physiologists considered as fully proved by the effects of madder upon the bones. They had by this means an opportunity of seeing the bones altered in colour, from the slightest tint to the deepest red; they could observe this gradually removed, until the bones had regained their natural whiteness; and explaining the whole process on the principle of deposition and absorption, they considered it as ocular demonstration of a most rapid change in the constituent elements of a part, of which, from its solidity, they could scarcely have believed it susceptible.

Probability that this explanation is erroneous.

I apprehend, however, that it is by giving an erroneous explanation of the phenomena; by supposing that a change in the osseous particles is denoted by an alteration in their colour, that physiologists have considered this fact as conclusive.—However indubitable and well supported may be the opinion, which attributes an imperceptible change to the various parts of the body, we shall, I believe discover upon a more close examination, that it is by no means supported by the appearances, which the bones display on the exhibition of madder root. The rapid change in their particles, which such appearances indicate, when explained in the common way, is completely at variance with all the processes performed by the bones, both in their healthy and diseased states. Thus we find the formation of the osseous matter, called Callus, for the

union of fractured bones, or the exfoliation of a part of a bone, are processes requiring a considerable length of time for their performance. In Dr. M'Donald's experiments, the formation of a regenerated bone required nearly six weeks; but during the same space of time, the bones of the same animal would be renewed several times, if the common explanation of the communication and disappearance of the tinge of madder were well founded. From these circumstances, I am led to believe that the appearances produced by the exhibition of madder, require another mode of explanation. That which I have to offer is not liable to the same objections, and is strongly supported by comparative experiments.

It was observed by Du Hamel, in his experiments, that the bones of animals which had been deeply tinged by madder, by long exposure to air lost their colour and became white.—

A simple explanation grounded on experiment.

It was this fact which suggested to me a simple explanation of the process. It occurred to me, that if any one of the component parts of the blood naturally exerted a stronger attraction for the colouring matter of madder, than the phosphate of lime, it might be deprived of the tint by a chemical power. In order to prove this, as far as I could by experiment, I took one dram of the phosphate of lime tinged, as in Dr. Rutherford's experiment, and exposed it for half an hour to the action of two ounces of fresh serum, at the temperature of 98 degrees. By this operation, the serum gradually acquired a red tinge, whilst the phosphate of lime was proportionably deprived of colour. In a comparative experiment, a similar quantity of tinged phosphate of lime was exposed to the action of distilled water under similar circumstances; but no change took place. The knowledge of this strong affinity in the serum for colouring matter, affords an easy and simple explanation of the effects of madder on the bones, upon the principle of chemical attraction.

The serum of blood has a stronger attraction for the colouring matter of madder than phosphate of lime has.

Thus, when an animal has madder mixed with its food, the blood becomes highly charged with it, and imparts the superabundant colouring matter to the phosphate of lime, contained in the bones already formed; as it circulates through them and moistens them throughout. But as soon as an animal has ceased to receive the madder, and the blood is freed from the colouring matter by the excretions, the serum then exerts its superior attraction, and by degrees entirely abstracts it from the

Hence the bones are dyed when much madder is in the system, and bleached by the serum when the quantity becomes less.

the phosphate of lime, and the bones resume their natural whiteness. In short, the bones are at one time dyed by the colouring matter, at another time bleached by the serum.

Phosphate while suspended does not strongly take the colouring matter.

Example in eggs.

Whilst I have attempted to explain the probable manner in which the bones, *already formed* in an animal, at one time receive, and at another are deprived of the colouring matter of madder, I by no means intend to assert that the phosphate of lime does not acquire a similar colour during its solution in the serum, or at the time it is precipitated from it to enter into the composition of the bones; the fact is indisputable. I have, however, found from some experiments lately made upon a hen during oviparation, that only a slight tinge can be communicated to the shell, formed whilst a large quantity of colouring matter is circulating with the blood. So slight indeed is the blush, that it would not be seen by a common observer, unless contrasted with a natural egg: which is probably the reason why it has, I believe, been denied by physiologists, that the shell of an egg is altered by the exhibition of madder. If this may be considered as a test of the quantity of colouring matter, which the phosphate attracts at the time it is separated from the blood, it forms another strong argument against the theory, which Dr. Rutherford, and all preceding physiologists have adopted; for, consistent with this fact, the bones should never exhibit more than a slight blush. When explained upon the principle of chemical attraction, we see that the phenomena, exhibited by the bones of an animal, by giving or withholding madder root, give no support to the opinion that the various parts of the body continually undergo an imperceptible change; and I consider it a fortunate circumstance for that doctrine that so simple an explanation of the effect of madder can be given. For whilst so specious a fact has been considered, by the highest authorities, as complete proof of the imperceptible renovation of parts; the rapid change in the constituent elements of the bones, which the communication and disappearance of the colour indicates, must have appeared astonishing to every physiologist. Of this I cannot give you a stronger instance than in the words of Mr. J. Bell. * Nothing," says he, " can be more curious than this continual renovation and change of parts even in the hardest bones. We are accustomed to say of the whole body, that it is daily

The doctrine of a rapid and continual change is not supported by the facts of bones tinged by the madder.

* Anatomy of the bones, &c. p. 15.

cl ; that the older particles are removed, and new ones their place; that the body is not now the same initial body, that it was; but it could not be easily believed that we speak only by guess concerning the softer parts, which we know for certain of the bones.—When madder is given to animals, withheld for some time and then given again, the colour appears in their bones, is removed, and appears again with such a sudden change, as proves a rapidity of deposition and absorption exceeding all likelihood or belief; all the bones are tinged in twenty-four hours; in two or three days their colour is very deep, and if the madder be left off but for a few days, the red colour is entirely removed.”

Although by this chemical explanation of the effect of madder upon the bones, the doctrine of the imperceptible change in the component parts of animal bodies, loses the support of a fact, which has, since its discovery, been universally considered as its strongest proof; nevertheless, indisputable arguments, derived from different sources, still place that doctrine amongst the best supported opinions in physiology.

V.

On Fairy Rings and the Waste of Fish in Scotland. By A. T.

To Mr. NICHOLSON.

SIR,

HAVING frequently noticed the fairy-rings your correspondent, M. Florian Jolly mentions in your Journal for February, I should be glad to know from him whether hares or rabbits abounded in Broadlands park, as I have generally observed these rings most prevalent, in light sandy soils, particularly among rabbit burrows. This species of soil from its dryness would be very unfavourable to the idea of these things being formed from a central heap of horse dung; besides, were this the cause of them, we should expect them to be always circular, or when not circular, that those parts most remote from the centre would appear not to have benefited so strongly from the manure as those which were nearer. I have generally observed that the rings were composed of a double circle, or rather a little circular path, the middle of which appeared to be

Observations
and inquiry
whether any
ring may not
have been made
by hares and
rabbits.

be trodden, and the edges grown up, and more in *igour* than any of the surrounding grass. I had occasion to remark one of those fairy-rings last summer: it was perfectly circular; and about ten feet in diameter, it was situated at the edge of a copse wood, and in a vicinity where there are abundance of both hares and rabbits; but what appeared to me most singular, was its being intersected exactly through the middle, by a well frequented foot path. The hare is rather given to gravity, the rabbit is more playful; but whether it is given to the amusement of *lounging in the ring*, *some* of your more informed correspondents may be enabled to inform you.

Fish is undoubtedly wasted in Scotland.

I OBSERVE some of your correspondents have got into a controversy respecting the waste of fish in Scotland. No doubt can exist upon that head; not however arising from the wasteful disposition of the natives, their delicacy in appetite or superabundance of provision, but from the want of a market for the consumption of their overplus. To talk of Aberdeen fishermen bringing fresh fish to Newcastle, Norwich, or Leeds, is as ridiculous as to propose taking them to Amsterdam, or London; for besides the difficulty of again making their own ports, they will constantly find an over-stocked market, as the same weather that permits them to fish will permit their neighbours to do the same. But the grand cause

The fisheries of Scotland are destroyed by the operation of the salt laws.

of all the waste is the horrible monopoly which their country labours under in respect to their salt laws, where for the sake of a few paltry pence, English salt is excluded under the severest penalties, although it can be delivered in any part of Scotland at one half the price that we are forced to pay for Scotch salt under the present circumstances. Give them salt at a cheap rate, if it does not permit them to export the fish, as that requires capital and new establishments, it would at least enable them to supply the interior; a thing as worthy the attention of the public as the supply of any other market I know.

Your most obedient
A. T.

March 23, 1803.

Letter.

VI.

Letter from AMICUS respecting the supposed Waste of Crab-Fish in Scotland.

TO MR. NICHOLSON.

SIR,

THE very respectable and distinguished rank which the Philosophical Journal holds among the periodical publications will at all times prevent its becoming the vehicle of unnecessary dispute or contradiction: yet as public information and utility is sometimes promoted by the correction of mistakes, when this is likely to be the case, any thing that can elucidate a fact either misrepresented or partially stated, is doubtless compatible with the spirit of your publication. In your 48th number it is stated by "an Enquirer" that the crab fishery is so productive about Arbroath that, after boiling them, the bodies of the crabs are thrown away, and the large claws only brought to table, of which the Enquirer says he has been a witness. The fact is literally true, but wants further explanation. It is well known to every person resident on the coasts where crab-fish are commonly to be had, that many of that species are scarcely eatable, being often found after boiling to contain hardly any thing but water. The writer of this article has repeatedly seen from twelve to twenty crabs boiled at one time, and every one of them, more or less, in the above situation. When this is the case, the meat of the great claws (although they still may be eaten) is also watery and insipid compared to those of a good crab, the body of which is filled with a very rich substance, which is so far from being thrown away, that it is in general esteemed a luxury, even where crabs are plenty. Some persons are, indeed, fond of the claws, who cannot eat the bodies at all; but these are only exceptions from general taste and common practice. The claws of a good crab (as has been already observed) are much firmer, more rich, and sweet to the taste than those of an inferior kind, which are by far the most abundant. The claws of the male are larger in proportion than those of the female: the male crab is also reckoned superior in quality, except for a very short period (in what time of the year I have not been able

to

to ascertain) when in the the opinion of some who pretend to be connoisseurs, the females are equal, or nearly so in the case.

Crabs are in season nine months in the year; May, June, and July are the only months in which they are not. Some piscatory epicures pretend to certain marks for distinguishing good crabs, but they are very far from being infallible; perhaps the most general distinction is, that a good crab has a shell of a dusky red colour, with a certain degree of roughness, particularly on the claws; while the bad ones have shells white, clear, smooth, and watery; but the distinction is much better understood from observation than any detailed account. Trusting that you, Mr. Editor, will have the goodness to insert this communication, and that your correspondent, "the Enquirer," will do me the justice to believe that my sole motive for troubling you was to give information, I am with esteem,

SIR,

Your most obedient servant,

AMICUS.

Atbroath, March 1, 1806.

VII.

Probability that the Hindoos were acquainted with Saturn's Ring.

To Mr. NICHOLSON.

SIR,

I TAKE the liberty of requesting the insertion of the following quotation in your Philosophical Journal, from the 11th vol. of Mr. Maurice's *Indian Antiquities*, page 600. If it does really mean the ring of the planet Saturn, perhaps some of your readers can explain how it could have been discovered by the Brahmins in such remote ages.

Yours respectfully,

April 7, 1806.

A. B. C.

Extract from
Maurice's *Indian
Antiquities*.

"I have already intimated in a former volume, that the circle formed around SANK (the Saturn of the Hindoos) by intertwining serpents, was probably intended to denote his RING. I have since had the figure engraved for the reader's inspection and decision. It is impossible to ascertain the exact age of the pictured image in the Pagoda, from which the portrait was taken;

taken; but probably both are of a very remote age, for the Indian pagodas are not fabrications of yesterday, nor in their conceptions and designs are they given to frequent vicissitude. Nor if Sani were thus designated in very antient periods, the fact proves that they must, by what means can scarcely be conjectured, have discovered the phenomenon of his ring, for what besides could that serpentine oval inclosing the body of Sani be intended to represent? That phenomenon however was not known in Europe till about the year 1628, when Galileo, with the first perfect telescope discovered, what he conceived to be two stars at the extreme parts of the planet, but which in reality proved to be the *angle* of that ring, the natural existence of which was afterwards demonstrated by Huygens and succeeding astronomers. The circumstance is not the least wonderful of those that occur in the discussion of Indian antiquities and literature. I have stated the fact, and engraved the image; I leave to abler judges the task of decision "

VIII.

Explanation of Time keepers constructed by Mr. Thomas Earnshaw: for which a Reward of Three Thousand Pounds was awarded by the Commissioners of Longitude. From the Communications made by him to the Commissioners.*

THE model, from which the annexed drawings were taken contains, besides the parts necessary to explain the nature of the Escapement, a box inclosing a spring, which when wound up communicates, by means of some more wheels, a force to the balance-wheel, sufficient, when the balance is put in motion, to keep it in action for some time. These wheels are contained between two brass plates, fastened together by four upright,

*Description of
the Escapement
of Mr. Earn-
shaw's time
piece.*

* The Escapement with a model was communicated in June, 1804, and a subsequent explanation in March, 1805. The former is here given, and so much of the latter as directly relates to the time-keepers. The latter paper is not otherwise abridged than by omitting certain observations upon other artists, and some general remarks which do not form part of the disclosure.

I have been solicitous to give as early an account as might be proper, of the Escapements of Mr. Earnshaw and Mr. Arnold
• which

Description of
the Escapement
of Mr. Barn-
shaw's time
piece.

upright pillars; the uppermost of these plates is that which is represented by Fig. 1st. plate XIII, where PQRS are the four screws that take into the heads of the four pillars above mentioned, and connect it to the remaining part of the model. The plate PQRS contains, however, the whole of the parts necessary for the present purpose. The side of this plate represented to view, is the undermost when fixed in the model; so that the figure represents this plate as taken off, with the side next to the balance laid upon a table, and the eye is supposed to be placed perpendicular over it.

In the plate PQRS is an opening, or a piece taken out, represented by TUVWXYZ. In this opening, the balance-wheel ABCD, pallet MSK, and part of the balance UV are seen. The balance-wheel is supported by two pieces of brass, ONH, OI; the piece ONH is screwed to the side of the plate nearest to view by a strong screw *t*, and made firm by small pins represented by $\pi \pi \pi \pi \pi \pi$; these pins are called steady pins; they are riveted fast into the supporting piece OH, and take into holes in the plate PQRS, made exactly to fit them. The part ON of this supporting piece is supposed to be raised above the part *t* H by a joint or bend at N; the other supporting piece OI is fastened to the opposite side of the plate; and between these two pieces the balance-wheel turns freely and steadily in the direction of the letters ABCD. The small wheel MSK is called the large pallet; it is a cylindrical piece of steel, having a notch or piece cut out of it at *l h r*; against the side of this notch is a square flat piece of ruby, or any hard stone, *h l*, ground and polished very smooth, and fixed fast into the pallet. The cylinder is so placed, with respect to the balance-wheel, that it may not be more than just clear of two adjoining teeth. EF is a long thin spring, which

(which last appears in No. 55 of our Journal) as they have been so highly distinguished by the national munificence. Some discussion of the important subject of time pieces may be seen in the *Philosophical Journal*, quarto series, Vol. I. 56, and Vol. II. 106. As I expect shortly to be favoured with a valuable communication respecting the original inventors of free Scapements and compensations, and may, according to circumstances, offer a few remarks on the subject myself, I have been careful in the first place to give the accounts of the above mentioned artists in their own words.

W. N.

which is made fast at one end, by being pinned into a stud, *G*, and made to bear gently against the head of an adjusting screw *m*; the other end is bent a little into the form of a hook; to this spring there is fixed another very slender spring at *y*, which projects to a small distance beyond it. This small spring lies on the side of the thick spring nearest to the balance-wheel. The adjusting screw, *m*, takes into a small brass-cock, α *p*, which is screwed fast to the plate PQRS by a strong screw at *p*. Upon the spring EF there is fixed a semi-cylindrical pin, which stands up perpendicular upon it, and of a sufficient length to fall between the teeth of the balance-wheel ABCD. This pin is called the locking-pallet, and is placed on the opposite side of the spring represented to view. Through the center of the cylindrical pallet MSK, a strong steel axis passes called the verge; the pallet is made fast to this axis, which also passes through the center of the balance, and is made fast to it; it has two fine pivots at its extremities, upon which it turns very freely, between two firm supporting pieces of brass screwed firmly, and made as permanent as possible, by steady pins to the principal plate PQRS; one of these pieces is represented in the figure by *w y L*; the part *w* is raised above the part *y L* by a bend or joint at *n*; the part *y L* being represented as fixed firm to the plate by the strong screw at *y*. This piece is called the potence, and is exactly similar to the other supporting piece, which is called the cock, that is similarly fixed to the opposite side of the plate and hid from the sight in the figure. A little above the cylindrical pallet MSK (as it appears in the figure) is fixed a small cylindrical piece of steel *in*, having a small part projecting out at *i*, through which the verge also passes; this is called the lifting pallet; it fixes upon the verge like a collar, and is made fast by a twist, so as to be set in any position with respect to the large pallet MSK. The balance lying below the plate PQRS, only the part UV is represented to view; the continuation of the position of the circumference, however, is represented by the dotted lines ULHV. The end EG of the long spring EF being made very slender, if a small force be applied at the point *o* to press that end out from the wheel ABCD, it easily yields in that direction, turning as if were upon a center at *G*; it is also made to slide in a groove made in this stud in such a manner that the end *o* may be placed at any required distance

Description of
the Escapement
of Mr. Earn-
shaw's time
piece.

Description of
the Escapement
of Mr. Earn-
shaw's time
piece.

from the center of the verge. Having described the several parts as they appear in the figure, we next come to their connexion or situation with respect to each other. Let the long spring EF be supposed to be so placed that the end of the slender spring *vi* may project a little way over the point of the lifting pallet *in*, but not so close but that the point of the pallet may pass by the hooked end of the spring EF without touching it; the head of the adjusting screw *m* is also supposed to bear gently on the inner side of the said spring EF, or that nearest to the wheel, and at the same time the locking pallet is so placed that one of the teeth D; of the balance-wheel, may just take hold of it. This pallet is not visible in its proper place in the figure, being covered from sight by the screw *m*, and part of the spring EF; its position is therefore represented by the dot *k*, on the opposite side of the wheel, having the tooth A just bearing up against it. From the above description of the several parts of the escapement, and their connexion with each other, it will be easy to see the mode of its action, which is as follows.

A force being supposed to be applied to the balance-wheel, so as to cause it to move round in the direction of the letters ABCD, one of the teeth, as D, will come up against the locking pallet (as represented at A, and the locking pallet by *k*). The wheel is then said to be locked, being prevented from moving forward by this pin. Let the balance be now supposed to rest in its quiescent position, and it will have the situation represented in the figure; the lifting point *i*, of the pallet *m*, will be just clear of the projecting end of the slender spring, the face *h* of the large pallet MSK will fall a little below the point of the tooth B, and the balance having its spiral or helical spring applied to it (which is here supposed on the other side of the plate PQRS, and of course not visible in the figure) remains perfectly at rest in this position. Now as the balance ULHV, and the two pallets MSK and *in*, are fixed fast to the verge, it is plain they must all move together; let therefore the balance be carried a little way round in the direction of the letters VULH; by this motion the end *i* of the lifting pallet *m* will be brought to press up against the projecting end of the slender spring, and as this spring is fixed on the side of the spring EF, nearest to the balance-wheel, the point *i* will press the two springs together out from the balance-wheel; then, as only the point of the tooth D (see its position at *k*) touches the locking

locking pallet, when the spring EF was at rest against the head of the screw *m*, it will, by the spring being pressed out from the tooth, have slipped off (for the locking pallet which was before supposed at *k*, will now be at *a*, clear of the tooth A of the balance-wheel; the wheel being now at liberty will move round by the force supposed to be applied to it; but as the point *i* of the lifting pallet moves on and presses out the spring, the point *l* of the large pallet approaches towards the point of the tooth B of the balance-wheel, so that when the spring EF is sufficiently pushed out to unlock the wheel, the point *l* of the large pallet will be got to *d*, and in this position the point of the tooth B of the balance-wheel will fall upon it (see Fig. 2,) where the tooth B is represented in contact with the pallet at *i*; at the same time the point of the tooth D has just dropt off from the locking pallet *m*; the force of the wheel being by this means applied to the top of the pallet *hi*, gives an increased momentum to the balance, and assists it in its motion in the same direction, and by the continued motion of the large pallet in the direction MSK the point of the tooth B, which keeps pressing and urging it forward, moves up towards the bottom of the face of the pallet towards *h*, until the plain flat surfaces of the tooth and pallet come into contact (see Fig 3); by this time the end *o* of the slender spring has dropt off from the point *i* of the lifting pallet, and the two springs have returned again into their quiescent position, the spring EF gently bearing against the head of the adjusting screw *m*, and the locking pallet in a position to receive the next tooth C of the balance-wheel; (see the position of the point of the lifting pallet at *n* Fig. 3, also the locking pallet at *m*, and the approaching tooth at C.) When the two surfaces of the tooth and pallet are thus in contact, the greatest force of the wheel is exerted upon the pallet, and of course upon the balance moving with it. The tooth still pressing against the face of the pallet, and the pallet moving in the direction MSK, it at last drops off, (see Fig 4, where *m* represents the position of the locking pallet, C the position of the tooth of the wheel just before it drops upon it, and *h* the position of the face of the large pallet, having the point of the tooth B just ready to leave it at *l*.) leaving the balance at perfect liberty to move on in the same direction in which it was going. Just as the point of the tooth B, which has been pressing the large pallet round, is ready to leave it,

the

Description of
the Escapement
of Mr. Earn-
shaw's time
piece.

the next tooth C of the wheel is almost in contact with the locking pallet *m* (see Fig. 4) so that the instant the tooth B drops off the wheel is again locked, and the action of that tooth upon the balance is finished. As the balance moves with the greatest freedom upon its pivots, the force of the tooth has given it a considerable velocity, so that the balance still keeps moving on in the same direction, after the pressure of the tooth is removed by slipping off from the pallet, until the force of the pendulum spring (which is not represented in the figure) being continually increased by being wound up, overcomes the momentum of the balance, which, for an instant of time, is then stationary, but immediately returns by the action of the pendulum spring, which exerts a considerable force upon it in unwinding itself. As the balance returns, the point *i* of the lifting pallet in passes by the ends of the two springs EF and γo , and, in passing by, pushes the projecting end, *o*, of the slender spring in towards the balance-wheel, until it has passed it; which, as soon as it has done, the projecting end *o* again returns and applies itself close to the hooked end of the spring EF, as before. The spring γo is made so slender, that it gives but little resistance to the balance, during the time the point *i* of the lifting pallet is passing it, and of course causes but little (if any) decrease in its momentum. During the time the point *i* of the lifting pallet is passing the small spring γo , the long spring EF remains steadily bearing against the head of the adjusting screw *m*, as the hooked end at *o* just lets the end of the lifting pallet pass by without touching of it. As the spring has now been continually acting upon the balance, from the extremity of its vibration in the direction MSK, it has given it the greatest velocity, when the point *i* of the lifting pallet is passing the end *o* of the slender spring; for at this instant the spring which was wound up by the contrary direction of the balance, is now unwound again, or in the same state as it was in its quiescent position at first, and of course has no effect upon the balance at all in either direction; but the balance having now all the velocity it could acquire from the unwinding of the spring, goes on in the direction UVHL, until the force of this spring again stops it and brings it back again, moving in the same direction as at first, with a considerable velocity. By this return of the balance, the point *i* of the lifting pallet comes up again to the projecting end *o* of the slender spring, pushes

puffies back the long spring EF, and unlocks the wheel; and another tooth falling upon the face of the pallet *hl* gives fresh energy to the balance: and thus the action is carried on as before.

The Escapement should be made in the following manner: Instructions for making the Escapement.
The pivots of the balance axis should be the size of the verge-pivots of a good common sized pocket watch, and of the shape of Fig. 5. Pl. XIII, which will greatly add to their strength, the extreme end, or acting part only being straight; the jewel hole should be as shallow as possible, so as not to endanger cutting the pivot, and the part of action of the hole made quite back with only a very shallow chamfer behind to retain the oil; deep holes are very bad, for when the oil becomes glutinous, it will make the pivots stick so as to prevent the balance from its usual vibration. The pallet should be half the diameter of the wheel, or a little larger, for if smaller, the wheel will then have too much action on it, which will increase friction most considerably, and likewise cause the balance to swing so much farther to clear the wheel; consequently a check in the motion of the balance may stop the watch. The face of the pallet should run in a line of equal distance between the centre of the pallet and its extremity, and not in a right line to its centre, that is an increase of friction, and a loss of that power which is obtained by the wheel acting on the extremity of the pallet: this is clearly proved by time, by the hole worn by the points of teeth in all pallets that run in a line to the centre. The scape wheel teeth should form the same direction as the face of the pallet, under cut for the said purpose of avoiding friction, and maintaining the power, and for safe locking. The points of the wheel teeth must not be rounded off, but left as sharp as possible. The pivots of the scape wheel are to be a very little larger than the balance pivots.

The wheel is locked by a spring instead of a detent with pivots, as the French have made them, for those pivots must have oil, and when the oil thickens then the spring of the pivot detents is so affected by it as to prevent the detent from falling into the wheel quick enough, the consequence of this is irregular time and stoppage of the watch, and if ever such a watch went well for twelve months, chance must have had by far the most hand in it. Detent with a spring joint.

When the spring is planted on the side of the wheel, as in How to place the detent, pallets, &c.
any escapements, the part on which the wheel rests should be a little

a little short of a right angle, so that the wheel may have a tendency to draw the spring into it, for if stopped the other way, or beyond a right angle it will have a tendency to push the spring out; in that case the wheel will have liberty to run; the wheel should take no more hold on the spring than just sufficient to stop it, for if more, friction will be increased. The small return spring should be as thin as possible at the end fastened to the other spring, but at the outer end a little thicker; the spring should be planted down as close to the wheel as to be just free of it. The discharging pallet about one-third, or near one-half the size of the large or main pallet, the face of it in a right line to the centre, the back of it a little rounding and off from the centre. Great care must be used in taking off the edges of this discharging piece, to make it round to prevent cutting the spring, nor can it be made too thin so it does not cut; the end of it nearest the balance should be a little more out from the centre of the balance axis than the lower part of it towards the potence, for counteracting the natural tendency of the spring downwards from the pressure of the scape wheel; and that part of the spring on which the wheel rests should be stopped a little down to give the wheel a tendency to force it up, to counteract the natural inclination the wheel has to draw it down by its pressure on it.

Construction of
the balance with
its compensation
weights, &c.

The balance is to be made of the best steel, and turned from its own centre to its proper size, then put it into a crucible with as much of the best brass as when melted will cover it. The brass melted will adhere to the steel (for if any other metal is used by way of folder, that watch cannot go well), then turn it to its proper thickness, and hollow it out so as to leave the steel rim about the thickness of a repeating spring to a small sized repeating watch, turn the brass to twice or near three times that thickness of steel, cross it out with only one arm straight across the centre, and at each end of the arm fix two screws opposite to each other through the rim of the balance to regulate the watch to time, the diameter of the heads of these screws about equal to the thickness of the balance, a little more or less is not material. The compensation weights should be made of the best brass and well hammered, and a groove turned to let the rim of balance into it, and this should be cut into fourteen equal parts by a wheel engine.

gine, then you will have seven pair of pieces of equal size and weight; two of these pieces being screwed on the rim of the balance at equal distances will produce an equilibrium, a balance in the full sense of the word, equal in all its parts. In making balances great care should be taken that they get no bruises or bendings, for if they get a bruise on one side so as to indent the metal, that part will be less affected by heat and cold than the other parts which have not received the same violence to close its pores.

To adjust the balance in heat and cold—put the watch into about 85 or 90 degrees of heat, by the common thermometer, mark down exactly how much it gains or loses in 12 hours, then put it into as severe cold as you can get for 12 hours, and if it gains one minute more in 12 hours in cold than in heat, move the compensation weights farther from the arm of the balance about $\frac{1}{8}$ of inch, and if it gains one minute more in 12 hours in heat than in cold move the weights $\frac{1}{8}$ of inch nearer to the arm of the balance, and so on in the like proportion, trying it again and again till you find the watch go the same in whatever change of heat or cold you put it,

Adjustment to keep time in all temperatures,

Much difficulty has fallen to the lot of watchmakers in the endeavour to make timekeepers go nearly the same in the different positions. I have had my share of this, but it is now over; by far the greatest part of this difficulty arises from the balance spring not being properly made. But if the spring is made, as I shall describe hereafter, you have only to make the balance of equal weight and it will go within a few seconds per day in all positions alike, and if it vibrates not more than one circle and a $\frac{1}{4}$, by applying a small matter of weight to that part of the balance which is downward when in the position that it loses most, will correct it with great accuracy; but if it vibrates more than one circle and a $\frac{1}{4}$, then it will require the weight to be above instead of below; and after the watch has been going a few months and its vibration shortens to one $\frac{1}{4}$ circle, then it will go worse and worse by reason of the weight being in the wrong place; therefore, to avoid this evil, it is absolutely necessary to confine the vibrations to one $\frac{1}{4}$ circle, which will produce the most steady performance. It is common for watchmakers to adore a timekeeper when they see it vibrate a circle and a half, or more, and form an opinion of its excellence from this only; but I know

—and in all positions,

Rule. If the balance be in poise it will go nearly alike in all positions. Correct it by adding weight to the lowest part when in losing position if the same vibration be about 200°; if more degrees then add to the upper part. Hence a moderate vibration is best.

know from experience what would be the consequence, and have been condemned, because, when I have seen such watches I said I saw enough to declare that it would not give very accurate performance.

Concerning the
balance spring.

Balance spring. To find out the invisible properties of this apparent simple part of the machine, has given much more trouble than all the rest, I despaired of bringing timekeepers to the state I have done, and unless those hidden properties are known to timekeeper makers, however well they may execute all other parts they will find their most sanguine expectations frustrated. I have seen watchmakers boast of their timekeepers going well for a month or two, and from the knowledge I had of the effects produced by the balance spring, I have told them that a month or two more would destroy their hopes. The cylindrical spring being in all its turns of equal distance from the centre, in course every turn will be of equal strength, and called isochronal, and believed that all vibrations whether long or short would be performed in the same time; but this is not true, for if a man is to go four miles in the same time as he has gone one mile, he cannot do it with the same power; no, he must have impelling force to quicken his motion, or he will be four times as long in doing it. Therefore instead of the spring being equal in all its parts, it must be made to increase in thickness to the outer end, in such proportion as will cause the balance when thrown to a greater distance to return so much the quicker to make them equal; by long perseverance I found how to make such springs, and then I thought I had got all I wished for. But cruel disappointment nearly broke my heart, for I found I had yet another difficulty to break down, as my watches with such perfect springs were continually losing on their rates. What farther to do I knew not, and I own I was nearly if not quite mad. But obstinate in the cause and resolving not to give it up but with life, perseverance came once more to my aid—and with still more unremitting study, which nearly finished me, before I applied the following remedy for the before mentioned evil, I found, in the course of reasoning on bodies, that watch springs relax and tire like the human frame, when kept constantly in motion, and this may be proved by the following experiment: let a watch that has been going a few months go down, let it be down for a week

It is made tapering.

Springs are subjected to a relaxation of force & regain it by rest.

or two, or more, then set it going, and if it be a good time-keeper so as not to be affected by the weather it will go some seconds per day faster than it did when it was let down, but it will again lose its quickness in a gradual manner gaining less and less till it comes to its former rate. Therefore finding that isochronal springs would not do—and likewise having made springs of such shape as would render long and short vibrations equal in time—constantly lose the longer the watch went, I then made them of such shape as to gain in the short vibrations about five or six seconds per day more than the long ones, this quantity could only be found by long experience, and the way I proved this was to try the rate of the watch with the balance vibrating about $\frac{1}{3}$ of circle, then tried its rate vibrating one circle and a $\frac{1}{2}$, and if the short vibrations go slower than the long ones that watch will lose on its rate, and if they are equal, it will likewise lose, but that only from relaxation, and if it gains in the short vibrations more than five or six seconds in twenty-four hours it will in the long run gain on its rate, but if not more than that quantity, and the timekeeper is perfect in heat and cold and every other part, the above properties will render it deserving of the name of a perfect timekeeper, and this is a principal cause of my timekeepers excelling all others, and this the principal cause of some of my timekeepers going better than others, though made by me, the springs of them being made to accord more exactly to the above proportions; and this is the cause which has enabled me to foretel what my timekeepers would do, which Dr. Maskelyne, Mr. Crosley, and others can testify. The above effect is produced as follows. I find the common relaxation of balance springs to be about five or six seconds per day on their rates in the course of a year, therefore if the short vibrations are made by the shape of the spring to go about that quantity faster than the long ones, and as the spring relaxes in going by time so the watch accumulates in dirt and thickening of the oil which shortens the vibrations, the short ones then being quicker, compensates for the evil of relaxation of the balance spring. From this it is plain, that the causes of error in timekeepers are not undefined and vague in their nature, which has been supposed; for when it is certain that all causes of error may be overcompensated we cannot despair of finding the medium, and which

This gradual effect causes a loss on the rate which may be compensated by giving greater speed to the shorter vibrations in the first construction.

which may be easily proved by examining the going of my timekeepers. It will there appear that what errors, they are subject to, arise from causes certain and natural, and in consequence may be corrected by art*.

IX.

Experimental Enquiry into the Proportion of the several Gases or Elastic Fluids, constituting the Atmosphere. By JOHN DALTON.

On the component parts of the atmosphere.

IN a former paper which I submitted to this society, "on the constitution of mixed gases," I adopted such proportions of the simple elastic fluids to constitute the atmosphere as were then current, not intending to warrant the accuracy of them all, as stated in the said paper; my principal object in that essay was, to point out the manner in which mixed elastic fluids exist together, and to insist upon what I think a very important and fundamental position in the doctrine of such fluids:—namely, that the elastic or repulsive power of each particle is confined to those of its own kind; and consequently the force of such fluid, retained in a given vessel, or gravitating, is the same in a separate as in a mixed state, depending upon its proper density and temperature. This principle accords with all experience, and I have no doubt will soon be perceived and acknowledged by chemists and philosophers in general; and its application will elucidate a variety of facts, which are otherwise involved in obscurity.

Objects of this essay.

1. To determine the weight of each sep. atmosphere.

—and the relative weights of the gases at the surface of the earth

The objects of the present essay are,

1. To determine the weight of each simple atmosphere, abstractedly; or, in other words, what part of the weight of the whole compound atmosphere is due to azote; what to oxygen, &c. &c.

2. To determine the relative weights of the different gases in a given volume of atmospheric air, such as it is at the earth's surface.

* To this communication Mr. Earnshaw has annexed two plates with descriptions, shewing the parts of his time-piece; all which, except those of the Escapement (which we have given) are capable of the same variations as those of any other good movements. He asserts that the best train for time keepers is 18,000; that the frame wheel for pocket ones should have 15 teeth, and for box ones 13 teeth.

3. To investigate the proportions of the gases to each other, — as well as at such as they ought to be found at different elevations above the earth's surface. different elevations.

To those who consider the atmosphere as a chemical compound, these three objects are but one; others, who adopt my hypothesis, will see they are essentially distinct. With respect to the first: It is obvious, that, on my hypothesis, the density and elastic force of each gas at the earth's surface, are the effects of the weight of the atmosphere of that gas solely, the different atmospheres not gravitating one upon another. Now each single atmosphere presses by its whole weight; which is measured by its spring and that by its volume. Whence the first object will be obtained by ascertaining what share of elastic force is due to each gas in a given volume of the compound atmosphere; or, which amounts to the same thing, by finding how much the given volume is diminished under a constant pressure, by the abstraction of each of its ingredients singly. Thus, if it should appear that by extracting the oxygenous gas from any mass of atmospheric air, the whole was diminished $\frac{1}{4}$ in bulk, still being subject to a pressure of 30 inches of mercury; then it ought to be inferred that the oxygenous atmosphere presses the earth with a force of six inches of mercury, &c. Take away one of the gases and the loss of volume represents its pressure and the weight of that atmosphere.

In order to ascertain the second point, it will be further necessary to obtain the specific gravity of each gas; that is, the relative weights of a given volume of each in a pure state, subject to the same pressure and temperature. For the weight of each gas in any given portion of atmospheric air, must be in the compound ratio of its force and specific gravity. The weights of each gas in given volume had from sp. gravity.

With respect to the third object, it may be observed, that those gases which are specifically the heaviest must decrease in density the quickest in ascending. If the earth's atmosphere had been a homogeneous elastic fluid of the same weight it is, but ten times the specific gravity, it might easily be demonstrated that no sensible portion of it could have arisen to the summits of the highest mountains. On the other hand, an atmosphere of hydrogenous gas of the same weight, would support a column of mercury nearly 29 inches on the summit of Mount Blanc. The proportions at different heights are obtained from the progression with each gas in the same manner as in computation for the whole atmosphere.

The several gases constantly found in every portion of atmospheric air, and in such quantities as are capable of being appreciated, are azotic, oxygenous, aqueous vapour, and carbonic acid. It is probable that hydrogenous gas also is constantly

stantly present; but in so small proportion as not to be detected by any test we are acquainted with; it must therefore be confounded in the large mass of azotic gas.

1. *On the weight of the Oxygenous and Azotic Atmospheres.*

Processes for determining the oxygen in the atmosphere.

1. with nitrous gas

2. with sulphuric acid

3. Explosion with hydr. gen.

4. Exposure to green sulphat of iron.

5. Burning phosphorus.

All produce the same result.

Various processes have been used to determine the quantity of oxygenous gas.

1. The mixture of nitrous gas and air over water.

2. Exposing the air to liquid sulphuret of potash or lime, with or without agitation.

3. Exploding hydrogen gas and air by electricity.

4. Exposing the air to a solution of green sulphat or muriat of iron in water, strongly impregnated with nitrous gas.

5. Burning phosphorus in the air.

In all these cases the oxygen enters into combination and loses its elasticity; and if the several processes be conducted skillfully, the results are precisely the same from all. In all parts of the earth and at every season of the year, the bulk of any given quantity of atmospheric air appears to be reduced nearly 21 per cent. by abstracting its oxygen. This fact, indeed, has not been generally admitted till lately; some chemists having found, as they apprehended, a great difference in the quantity of oxygen in the air at different times and places; on some occasions 20 per cent. and on others 30, and more of oxygen are said to have been found. Thus I have no doubt was owing to their not understanding the nature of the operation and of the circumstances influencing it. Indeed it is difficult to see, on any hypothesis, how a disproportion of the two elements should ever subsist in the atmosphere.

The oxygen and azote are not separable.

The first process with nitrous gas has discredited, as here perfectly ed.

As the first of the processes above-mentioned has been much discredited by late authors, and as it appears from my experience to be not only the most elegant and expeditious of all the methods hitherto used, but also as correct as any of them, when properly conducted, I shall, on this occasion, animadvert upon it.

Instructions for the process.

Pure nitrous gas

1. Nitrous gas may be obtained pure by nitric acid diluted with an equal bulk of water poured upon copper or mercury; little or no artificial heat should be applied. The last product of gas thus obtained, does not contain any sensible portion of azotic gas: at least it may easily be got with less than two or three per cent. of that gas: It is probably nearly free from nitrous oxide also, when thus obtained.

2. If 100 measures of common air be put to 36 of pure nitrous gas in a tube 3-10th of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 measures, and exhibit no signs of either oxygenous or nitrous gas.

Mixture 100 air and 36 n. gas in a narrow tube. Residue about 80 azote.

3. If 100 measures of common air be admitted to 72 of nitrous gas in a wide vessel over water, such as to form a thin stratum of air, and an immediate momentary agitation be used, there will, as before, be found 79 or 80 measures of pure azotic gas for a residuum.

Mixture 100 air and 72 n. gas in a wide vessel with agitation. Residue as before 80 azote.

4. If, in the last experiment, less than 72 measures of nitrous gas be used, there will be a residuum containing oxygenous gas; if more, then some residuary nitrous gas will be found.

Intermediate proportion of n. gas leaves either n. gas or oxygen with the azotes.

These facts clearly point out the theory of the process: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former case *nitric acid* is the result; in the latter *nitrous acid*: but as both these may be formed at the same time, one part of the oxygen going to one of nitrous gas, and another to two, the quantity of nitrous gas absorbed should be variable; from 36 to 72 per cent. for common air. This is the principal cause of that diversity which has so much appeared in the results of chemists on this subject. In fact, all the gradation in quantity of nitrous gas from 36 to 72 may actually be observed with atmospheric air of the same purity; the wider the tube or vessel the mixture is made in, the quicker the combination is effected, and the more exposed to water, the greater is the quantity of *nitrous acid* and the less of *nitric* that is formed.

Theory of the process. In the first case nitric gas was formed; in the latter nitrous.

To use nitrous gas for the purpose of eudiometry therefore, we must attempt to form *nitric acid* or *nitrous* wholly, and without a mixture of the other. Of these the former appears from my experiments to be most easily and most accurately effected. In order to this a narrow tube is necessary; one that is just wide enough to let air pass water without requiring the tube to be agitated, is best. Let little more nitrous gas than is sufficient to form *nitric acid* be admitted to the oxygenous gas; let no agitation be used; and as soon as the diminution appears to be over for a moment let the residuary gas be transferred to another tube, and it will remain without any further diminution of consequence. Then $\frac{1}{3}$ of the loss will be due

Practical remark. Operate so as to form the nitric gas.

to oxygen. The transferring is necessary to prevent the nitric acid formed and combined with the water, from absorbing the remainder of the nitrous gas to form nitrous acid.

Method with sulphuret.

Sulphuret of lime is a good test of the proportion of oxygen in a given mixture, provided the liquid be not more than 20 or 30 per cent. for the gas (atmospheric air); if the liquid exceed this, there is a portion of azotic gas imbibed somewhat uncertain in quantity.

Volta's method.

Volta's eudiometer is very accurate as well as elegant and expeditious: according to Monge, 100 oxygen require 196 measures of hydrogen; according to Davy 192; but from the most attentive observations of my own, 185 are sufficient. In atmospheric air I always find 60 per cent. diminution when fired with an excess of hydrogen; that is, 100 common air with 60 hydrogen, become 100 after the explosion, and no oxygen is found in the residuum; here 21 oxygen take 39 hydrogen.

2. Of the Weight of the Aqueous Vapour Atmosphere.

To find the weight of aqueous vapour in the atmosphere.

I have, in a former essay, (Manchester Mem. vol. 5. p. 2, page 559.) given a table of the force of vapour in *vacuo* for every degree of temperature, determined by experiment; and in the sequel of the essay, have shewn that the force of vapour in the atmosphere is the very same as in *vacuo*, when they are both at their utmost for any given temperature. To find the force of aqueous vapour in the atmosphere, therefore, we have nothing more to do than to find that degree of cold at which it begins to be condensed, and opposite to it in the table abovementioned, will be found the force of vapour. From the various facts mentioned in the essay it is obvious, that vapour contracts no chemical union with any of the gases in the atmosphere; this fact has since been enforced in the *Annales de Chimie*, vol. xlii. by Clement and Desorme.

M. De Saussure found by an excellent experiment, that dry air of 64° will admit so much vapour as to increase its elasticity, $\frac{1}{44}$. This I have repeated nearly in his manner, and found a similar result. But the table he has given us of aqueous vapour at other temperatures is very far wrong, especially at temperatures distant from 64°. The numbers were not the result of direct experiment, like the one above. If we could obtain the temperatures of all parts of the earth's surface,

surface, for any given time, a mean of them would probably be 57° or 58° . Now if we may suppose the force of vapour equivalent to that of 55° , at a medium, it will, from the table, be \approx to .449 of mercury; or, nearly $\frac{1}{2}$ of the whole atmosphere. ^{It is on an average about one-seventieth.} This it will be perceived is calculated to be the weight of vapour in the whole atmosphere of the earth. If that incumbent over any place at any time be required, it may be found as directed above.

3. Of the Weight of the Carbonic Acid Atmosphere.

From some observations of Humboldt, I was led to expect about $\frac{1}{1460}$ part of the weight of the atmosphere to be carbonic acid gas: but I soon found that the proportion was immensely over-rated. From repeated experiments, all nearly agreeing in their results, and made at different seasons of the year, I have found, that if a glass vessel filled with 102,400 grains of rain water be emptied in the open air, and 125 grains of strong lime water be poured in, and the mouth then closed; by sufficient time and agitation, the whole of the lime water is just saturated by the acid gas it finds in that volume of air. But 125 grains of the lime water used require 70 grain measures of carbonic acid gas to saturate it: therefore, the 102,400 grain measures of common air contain 70 of carbonic acid; or $\frac{1}{1460}$ of the whole. The weight of the carbonic acid atmosphere then is to that of the whole compound as 1:1460; but the weight of carbonic acid gas in a given portion of air at the earth's surface, is nearly $\frac{1}{1460}$ of the whole; because the specific gravity of the gas is $1\frac{1}{2}$ that of common air. I have since found that the air in an assembly, in which two hundred people had breathed for two hours, with the windows and doors shut, contained little more than 1 percent. of carbonic acid gas.

Having now determined the force with which each atmosphere presses on the earth's surface, or in other words, its weight; it remains next to enquire into their specific gravities.

Specific gravi-
ties of gases.

These may be seen in the following Table.

Atmospheric air	- - - - -	1.000
Azotic gas	- - - - -	.966
Oxygenous gas	- - - - -	1.127
Carbonic acid gas	- - - - -	1.500
Aqueous vapour	- - - - -	.700*
Hydrogenous gas	- - - - -	.077*

Kirwan and Lavoisier are my authorities for these numbers, except oxygenous gas and aqueous vapour. For the former I am indebted to Mr. Davy's Chemical Researches; his number is something greater than theirs: I prefer it, because, being determined with at least equal attention to accuracy with the others, it has this further claim for credit, that 21 parts of gas of this specific gravity, mixed with 79 parts of azotic gas, make a compound of exactly the same specific gravity as the atmosphere, as they evidently ought to do, setting aside the unfounded notion of their forming a *chemical* compound. The specific gravity of aqueous vapour I have determined myself both by analytic and synthetic methods, after the manner of De Saussure; that is, by abstracting aqueous vapour of a known force from a given quantity of air, and weighing the water obtained—and admitting a given weight of water to dry air and comparing the loss with the increased elasticity. De Saussure makes the specific gravity to be .71 or .73; but he used caustic alkali as the absorbent, which would extract the carbonic acid as well as the aqueous vapour from the air. From the experiments of Piclet and Watt, I deduce the specific gravity of aqueous vapour to be .61 and .67 respectively. Upon the whole, therefore, it is probable that .7 is very nearly accurate.

We have now sufficient data to form tables answering to the two first objects of our enquiry.

* The specific gravity of hydrogen must be rated too low: if 100 oxygen require 185 hydrogen by measure, according to this 89 oxygen would require only 11 hydrogen to form water; whereas 89 require 15. Hydrogen ought to be found about $\frac{1}{16}$ part of the weight of common air.

I. *Table of the Weights of the different Gases constituting the Atmosphere.*

	Inch of Mercury.	Absolute weights of the different gases in the whole atmo- sphere.
Azotic gas - - - -	23.36	
Oxygenous gas - - - -	6.18	
Aqueous vapour - - - -	.44	
Carbonic acid gas - - - -	.02	
	<hr/> 30.00	

II. *Table of the proportional Weights of the different Gases in a given Volume of Atmospheric Air, taken at the Surface of the Earth.*

	per cent.	Weights of the different gases in equal bulks at the earth's surface.
Azotic gas - - - -	75.55	
Oxygenous gas - - - -	23.32	
Aqueous vapour - - - -	1.03*	
Carbonic acid gas - - - -	.10	
	<hr/> 100.00	

III. *On the Proportion of Gases at different Elevations.*

M. Berthollet seems to think that the lower strata of the atmosphere ought to contain more oxygen than the upper, because of the greater specific gravity of oxygenous gas, and the slight affinity of the two gases for each other. (See *Annal. de Chimie*, Tom. 34. page 85.) As I am unable to conceive even the possibility of two gases being held together by affinity, unless their particles unite so as to form one centre of repulsion out of two or more (in which case they become one gas) I cannot see why rarefaction should either decrease or increase this supposed affinity. I have little doubt, however, as to the fact of oxygenous gas observing a diminishing ratio in ascending; for, the atmospheres being independent on each other, their densities at different heights must be regulated by their specific gravities. Hence, if we take the azotic atmo-

Computation of the proportion of gases above the earth's surface.

It is not notably different at any accessible height.

* The proportion of aqueous vapour must be understood to be variable for any one place: the others are permanent or nearly so.

sphere as a standard, the oxygenous and the carbonic acid will observe a decreasing ratio to it in ascending, and the aqueous vapour an increasing one. The specific gravity of oxygenous and azotic gases being as seven to six nearly, their diminution in density will be the same at heights reciprocally as their specific gravities. Hence it would be found, that at the height of Mount Blanc (nearly three English miles) the ratio of oxygenous gas to azotic in a given volume of air, would be nearly as 20 to 80;—consequently it follows that at any ordinary heights the difference in the proportions will be scarcely if it all perceptible*.

X.

Observation which indicates a spontaneous Decomposition of nitrous Acid and Formation of Ammonia. By D. A.

To Mr. NICHOLSON.

SIR,

Decomposition
of nitrous acid.

I SEND you a statement of the following fact, in case it may not hitherto have been observed: it seems to shew the mutual decomposition of nitrous acid and atmospheric air; but the explanation of the theory I will leave to you, or some of your learned correspondents. A phial of bright orange coloured nitrous acid, so loosely stoppered that bubbles of gas escaped every five or ten minutes, having stood within a few inches of a bottle of muriatic acid, closely stoppered for above a twelve month, my attention was attracted by observing a white incrustation of salts upon the label paper of the last mentioned phial. To determine their nature, dissolved them in distilled water; dropped a little nitric acid in, to saturate any uncombined alkalies; then with nitrate of silver, a copious precipitate was formed, which indicates the muriatic to be the acid: when I saturated the acids with pure potash, the

* Air brought from the summit of Helvelyn, in Cumberland (1100 yards above the sea—Barometer being 26,60) in July 1804, gave no perceptible difference from the air taken in Manchester.—Mr. Gay-Lussac determines the constitution of air brought from an elevation of four miles to be the same as that at the earth's surface.

smell

smell resembled ammonia; but owing to the solution being so extremely weak, was scarce perceptible; but on a finger being dipped into it, and held near a stopper, moistened with muriatic acid, evidently produced a white cloud, which disappeared upon withdrawing the acid, and re-appeared on its approach; which test alone I think may be sufficient to prove ammonia to have been the base. I may observe, that the salts were formed only on that part of the table on which some muriatic acid had been spilt; the neck of the nitrous acid phial was covered with a moisture, which had a considerable ammoniacal smell, and exhibited the same appearances with the moistened stopper, and was therefore uncombined ammonia, and seems to shew that the presence of the muriatic acid was not necessary for its formation. I have endeavoured to be as concise as possible, and remain

Your constant reader,

April 17, 1806.

D. A.

SCIENTIFIC NEWS.

Note on the Porcelain of Reaumur Communicated by Veau de Launai.*

M. PECARD of Tours, manufacturer of Rouen stone ware, has repeated in his furnace Reaumur's experiment of transforming glass into porcelain; mentioned in the memoir of the Academy of Sciences, for the year 1739, p. 370. M. Pecard obtained a devitrification as complete within as without. His experiment was made upon a common glass bottle from the Ancenis Foundry. The bottle was filled with Nevers sand, and deposited in a fagger, which was afterwards filled up with the same sort of sand. The fagger or case was placed with others, containing earthenware in the chimney or upper part of the furnace, and heated as usual. When the operation was finished, and the furnace was sufficiently cooled, the bottle was taken from its bed of sand in the fagger, and emptied of its contents. The bottle had undergone no alteration of shape; but its green colour and transparency were exchanged for milky opacity, equally spread over all parts of the bottle. In this, his first ex-

Reaumur's
porcelain made
in an improved
state.

* Journal de Physique, Vol. LXI. p. 401.

periment,

periment, M. Pecard has obtained a much more equal devitrification than that procured by Reaumur; who remarks in his memoir, that he thinks it not impossible, that this point of equality between the internal and external parts may be obtained.

This substance is much harder than glass; it readily gives a spark with steel; and from the advantages it seems to hold forth in many respects deserves to be made an object of investigation.

Darcet applies the glass porcelain to useful purposes.

A distinguished chemist, who pursues the steps of his father, whose name will be ever dear to the sciences, and to those who cultivate them, M. Darcet, has already made several experiments on this interesting subject, which form part of a work not yet completed. He has made crucibles of this substance, exceeding the hardness of flint; also capsules and other articles which easily support the fire, and are not subject to the power of re-agents, such as sulphuric acid, &c. The little cost of the materials whereof these vessels, &c. are fabricated, induce a hope that the labours of Reaumur on this subject will be resumed, and carried on in a way that will be of utility in different arts.

Anatomical Work.

AN extensive work on the anatomy of the organs of hearing in animals, generally, together with the physiology of these several parts, and a series of acoustic phenomena intended to elucidate the subject, is in forwardness for publication this spring by Anthony Carlisle, F. R. S. F. L. S. and surgeon to the Westminster Hospital.

Fig. 1. *Horizontal Component.*

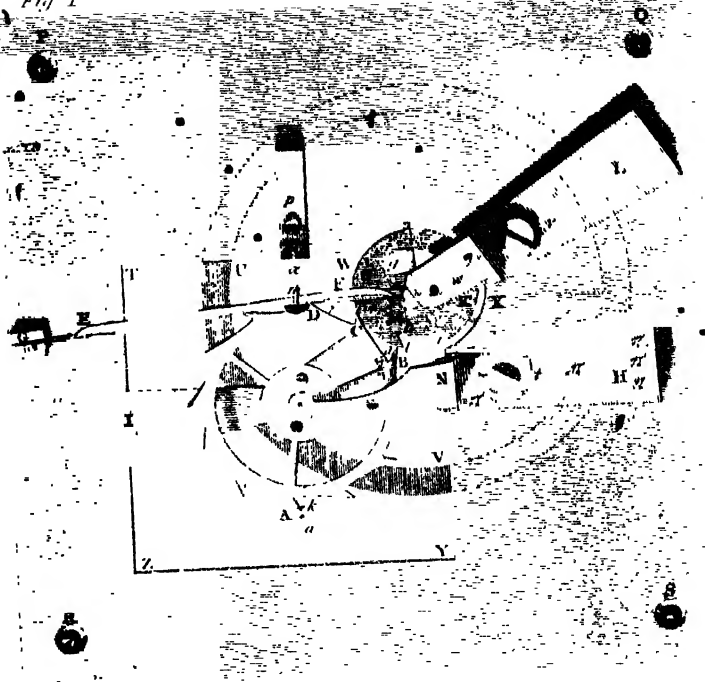


Fig. 2.



Fig. 3.

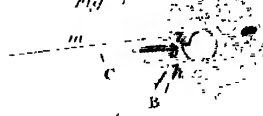


Fig. 4.

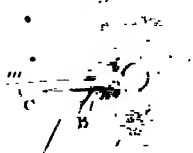


Fig. 5.



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THE END OF THE THIRTEENTH VOLUME.

Printed by W. Stratford, Crown-Court, Tenny-Bale.

